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# ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

*NATURAL HISTORY,*

AGRICULTURE, AND THE ARTS.

---

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IMPERIAL MEDICO-CHIRURGICAL ACADEMY OF PETERSBURGH.

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VOL. XIV



JULY TO DECEMBER, 1819.

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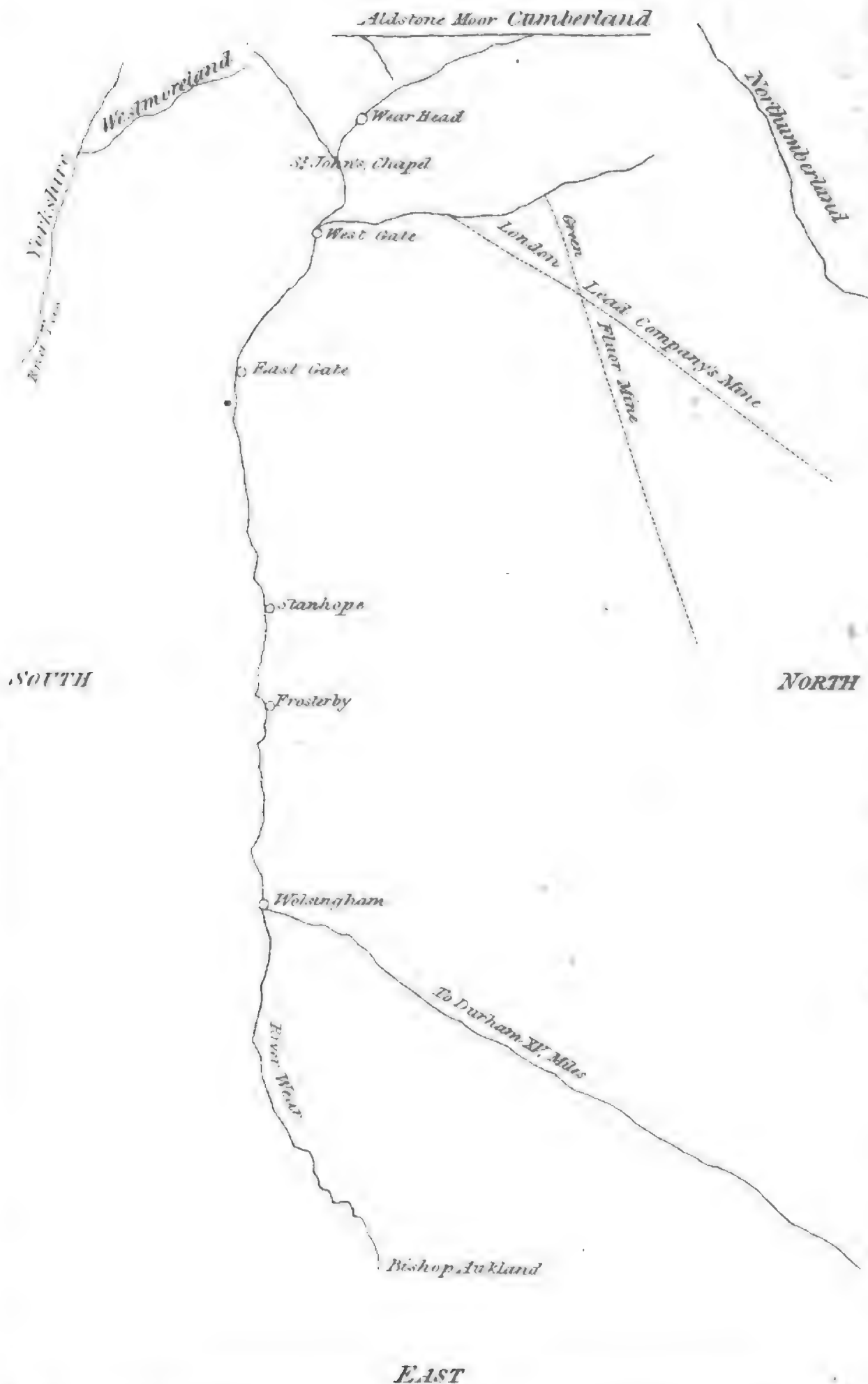
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## ERRATUM IN VOL. XIV.

In Mr. Brooke's paper, "On the Measurement of the Angles of Crystals," p. 455,  
    line 6, for 15' read 45'.







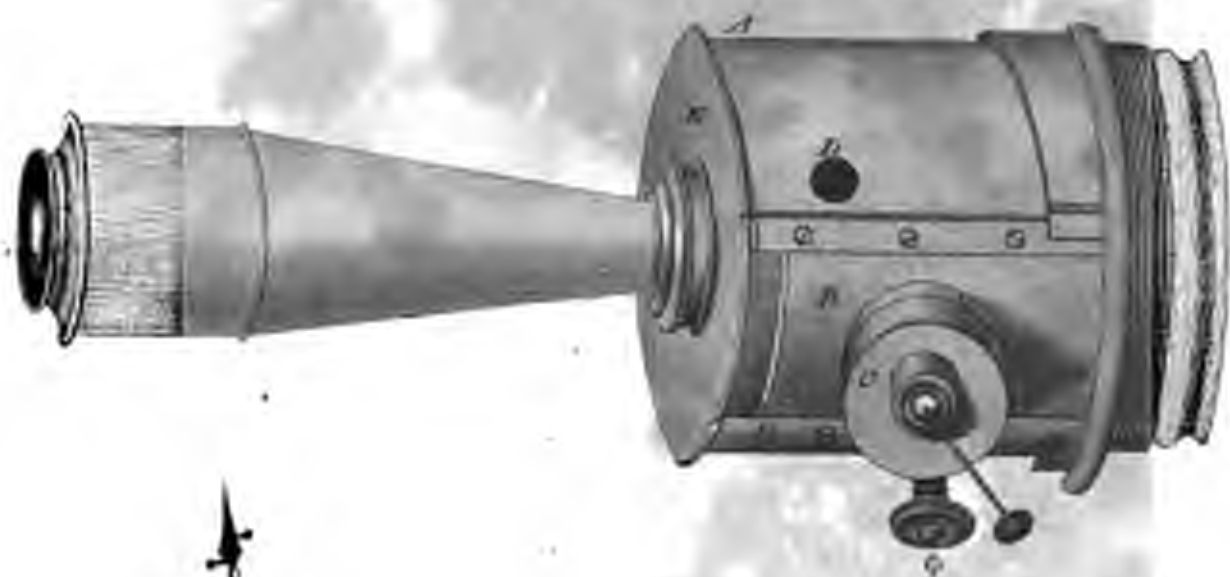


Fig P.

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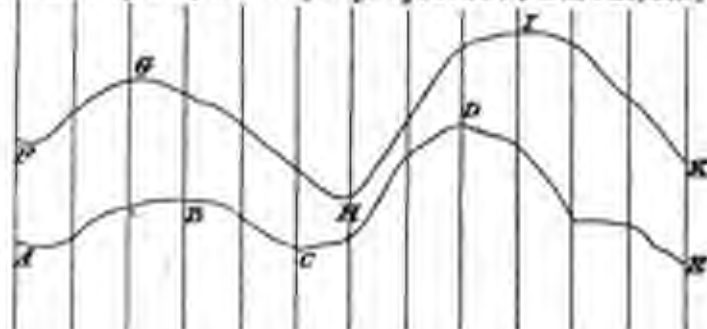
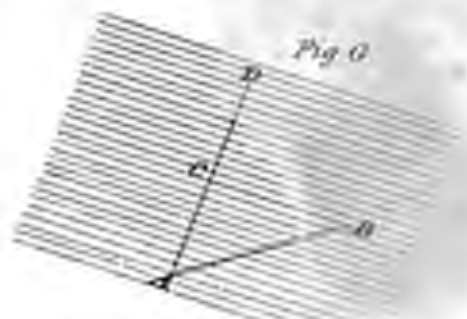


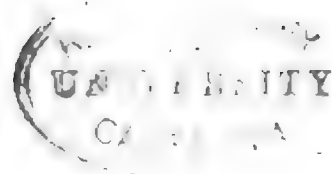
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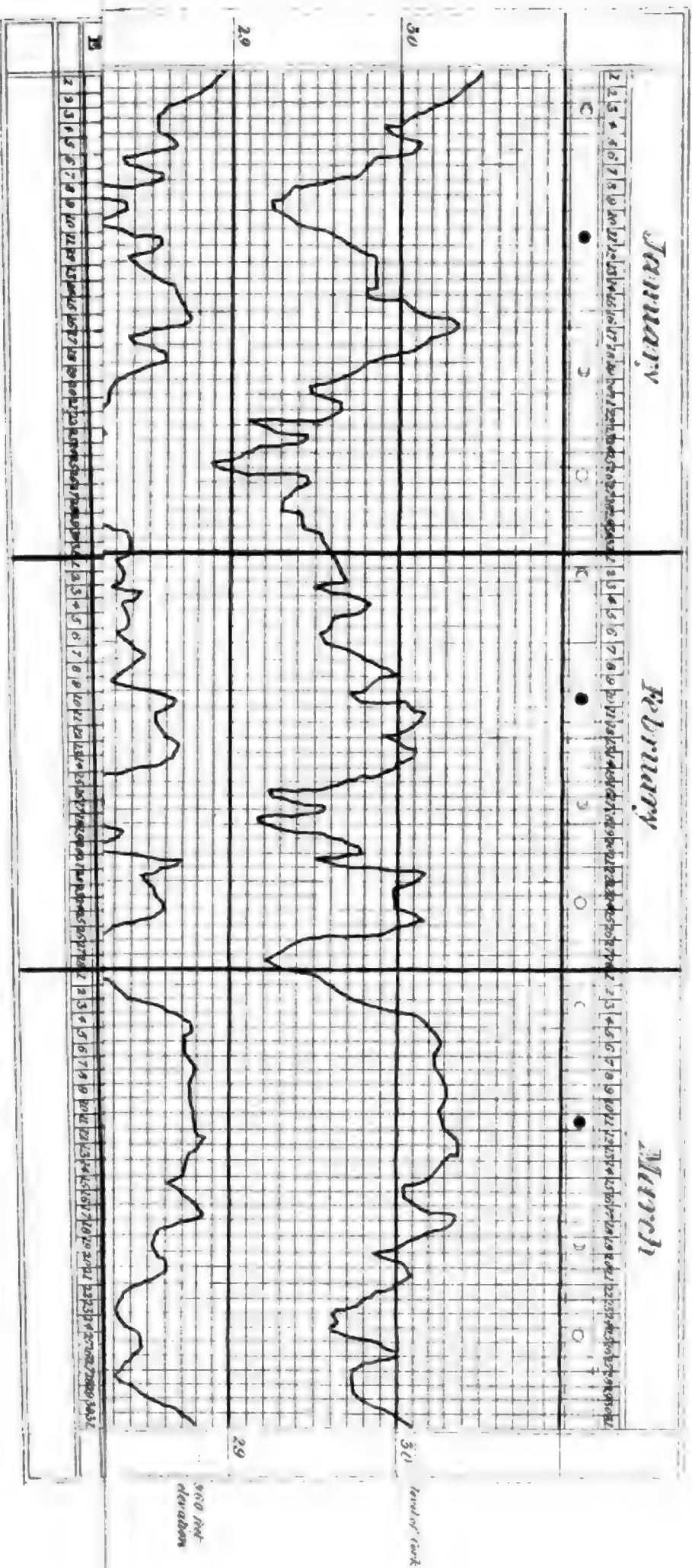






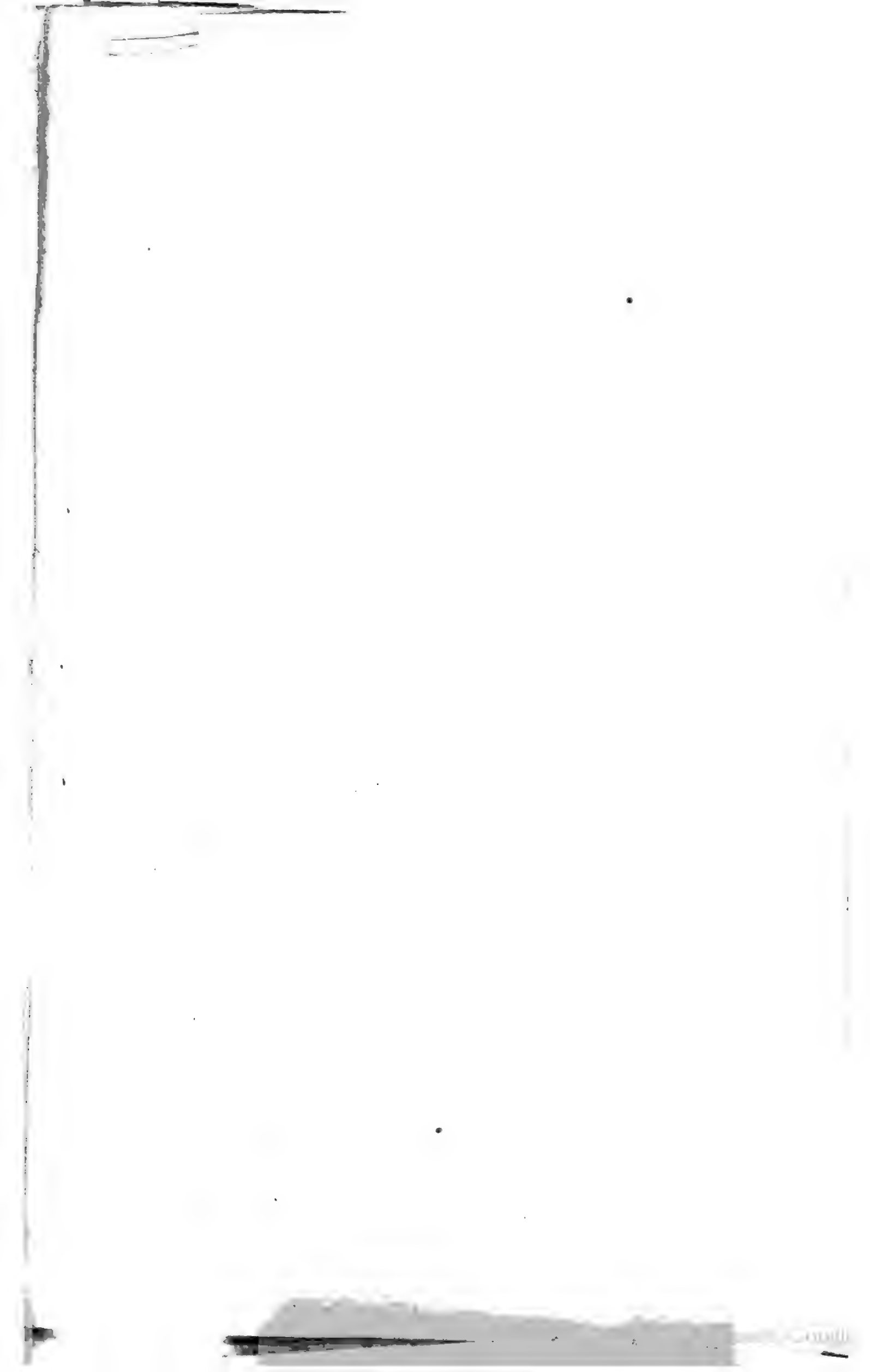


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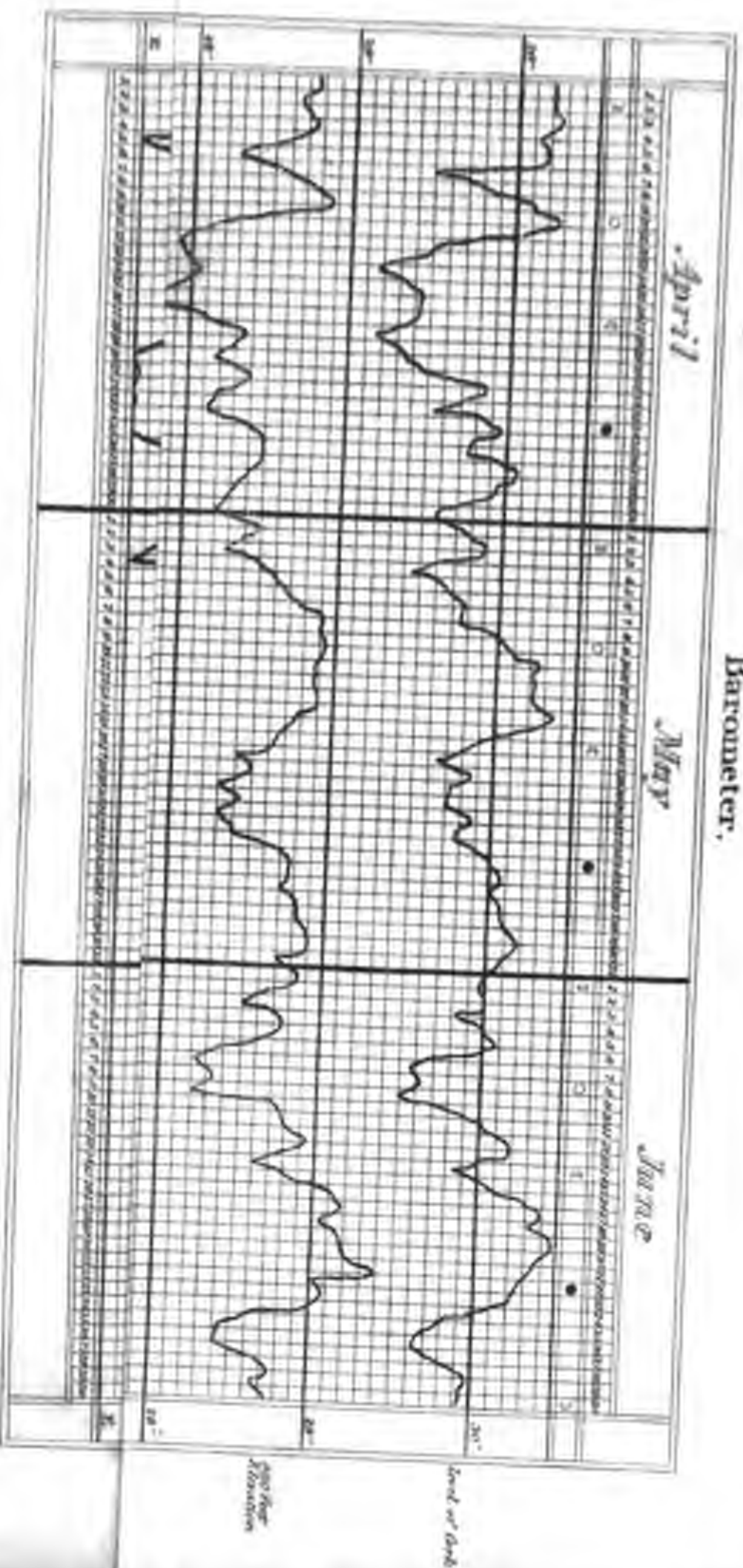


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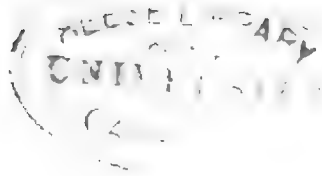




# Barometer.







XCIX.

Fig. 1.

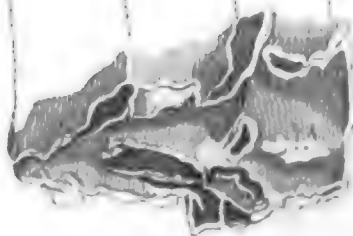
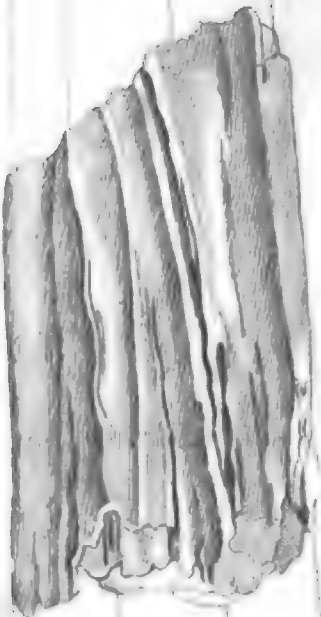
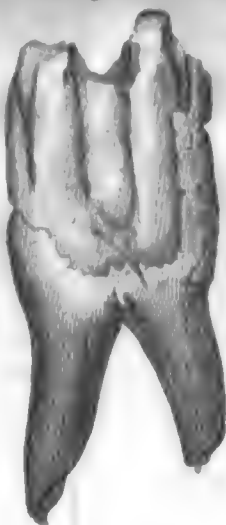


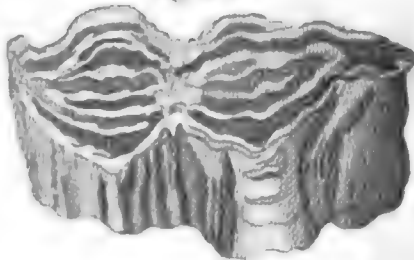
Fig. 6. a.



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Fig. 5. a



b



Fig. 2.

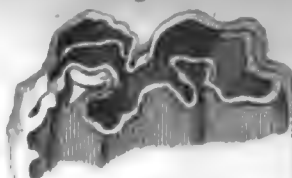


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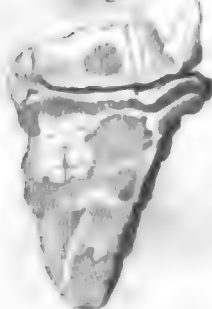
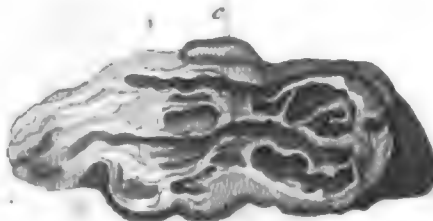
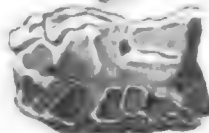


Fig. 3.





END

ANNALS  
OF  
PHILOSOPHY.

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JULY, 1819.

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ARTICLE I.

*Biographical Notice of the late Archibald Bruce, M.D. Professor of Materia Medica and Mineralogy in the Medical Institution of the State of New York, and Queen's College, New Jersey, and Member of various learned Societies in America and Europe.\**

**DOCTOR ARCHIBALD BRUCE** (the subject of this memoir), was a native of the city of New York, in North America. He was born in the month of February, in the year 1777. His father was at that time at the head of the medical department of the British army (then stationed at New York) to which he had been attached from his youth, having been many years previously resident at New York, as surgeon to the artillery department; where he was married, in or about the year 1767, to Judith, a daughter of Nicholas Bayard, formerly of the same city, at that time the widow of Jeremiah Van Renselaer of Greenbush; by whom he had another son (who died an officer in the British army in Ireland), and a daughter (who died while a child).

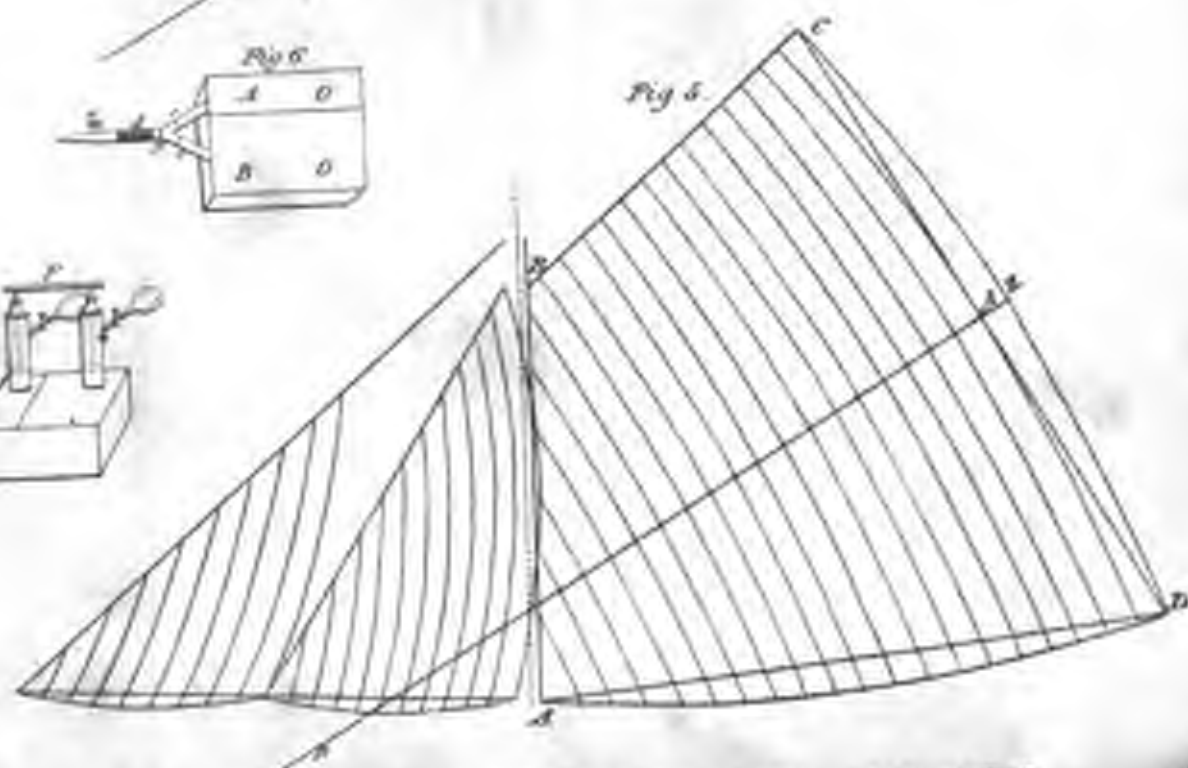
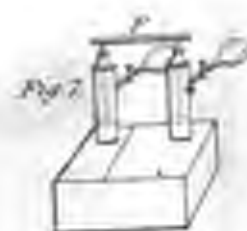
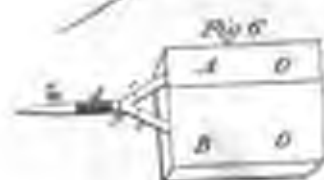
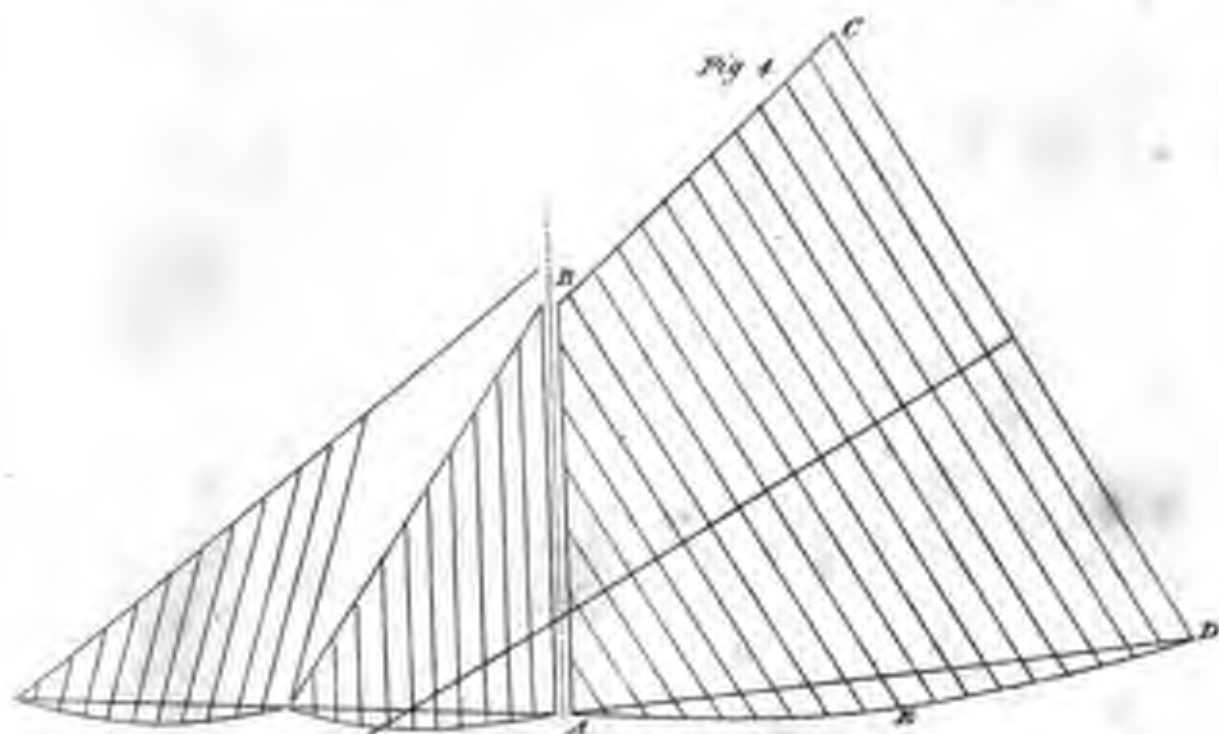
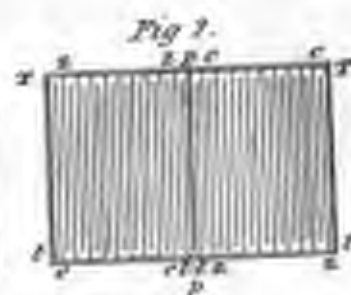
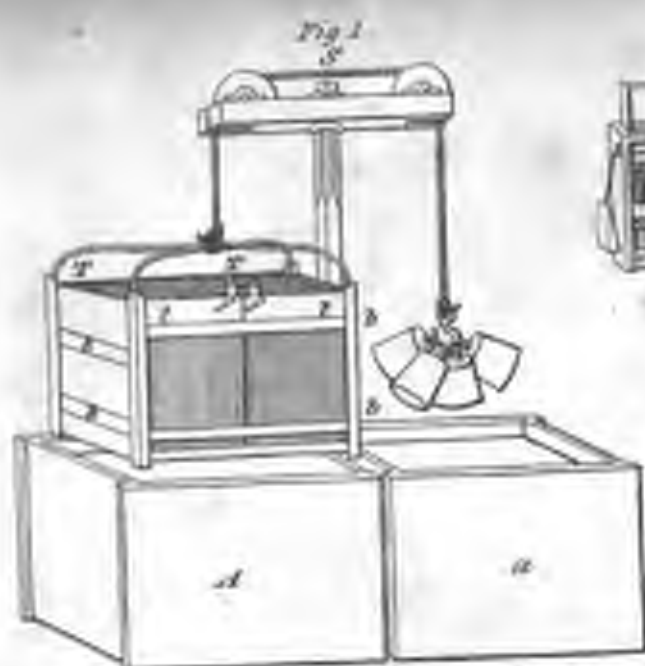
William Bruce (the father above-mentioned) and his brother Archibald, together with a sister, were natives of the town of Dumfries, in Scotland, where their father was many years resident as the parochial clergyman; and so continued until his decease, much respected.

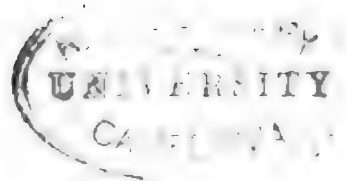
Both sons applied themselves to the science of medicine and surgery. William, as above stated, became a physician in the

\* From Silliman's American Journal of Science, vol. i. p. 299.

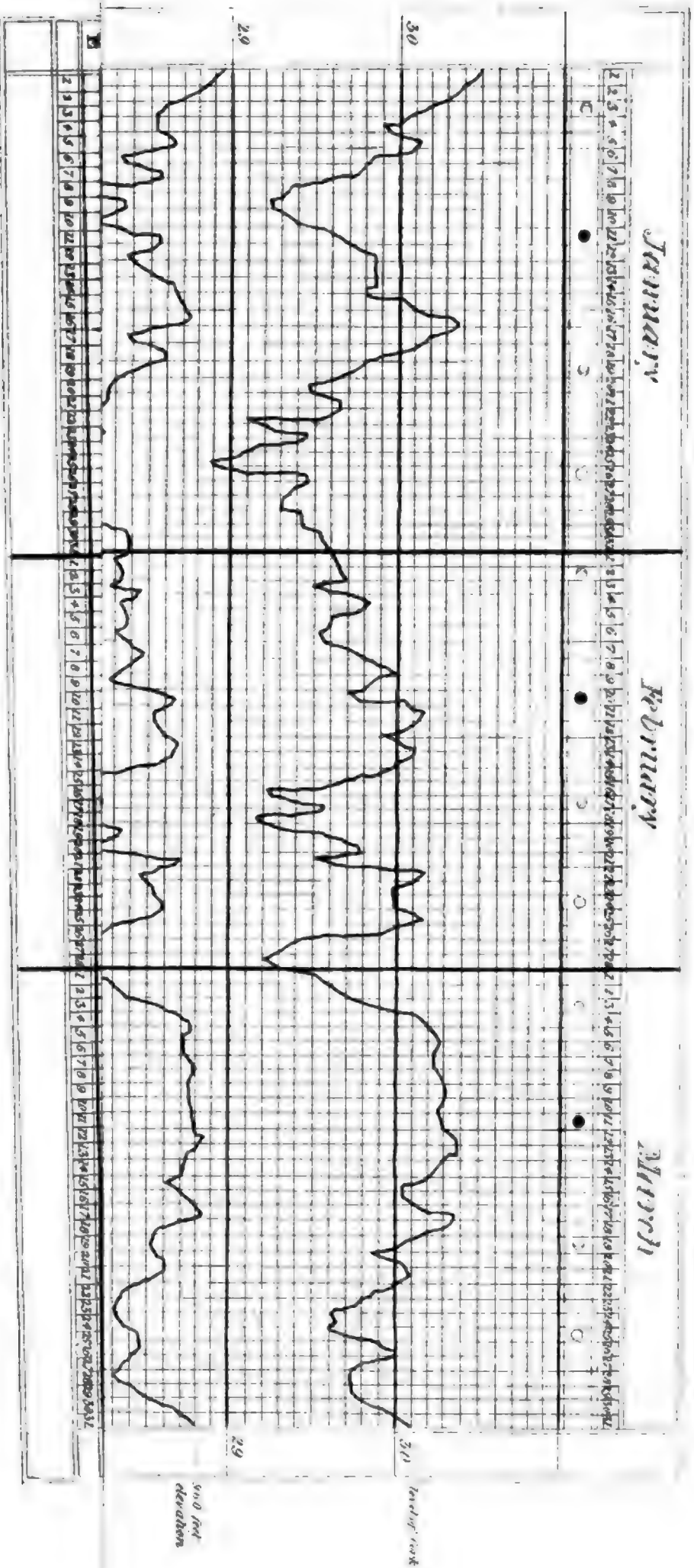








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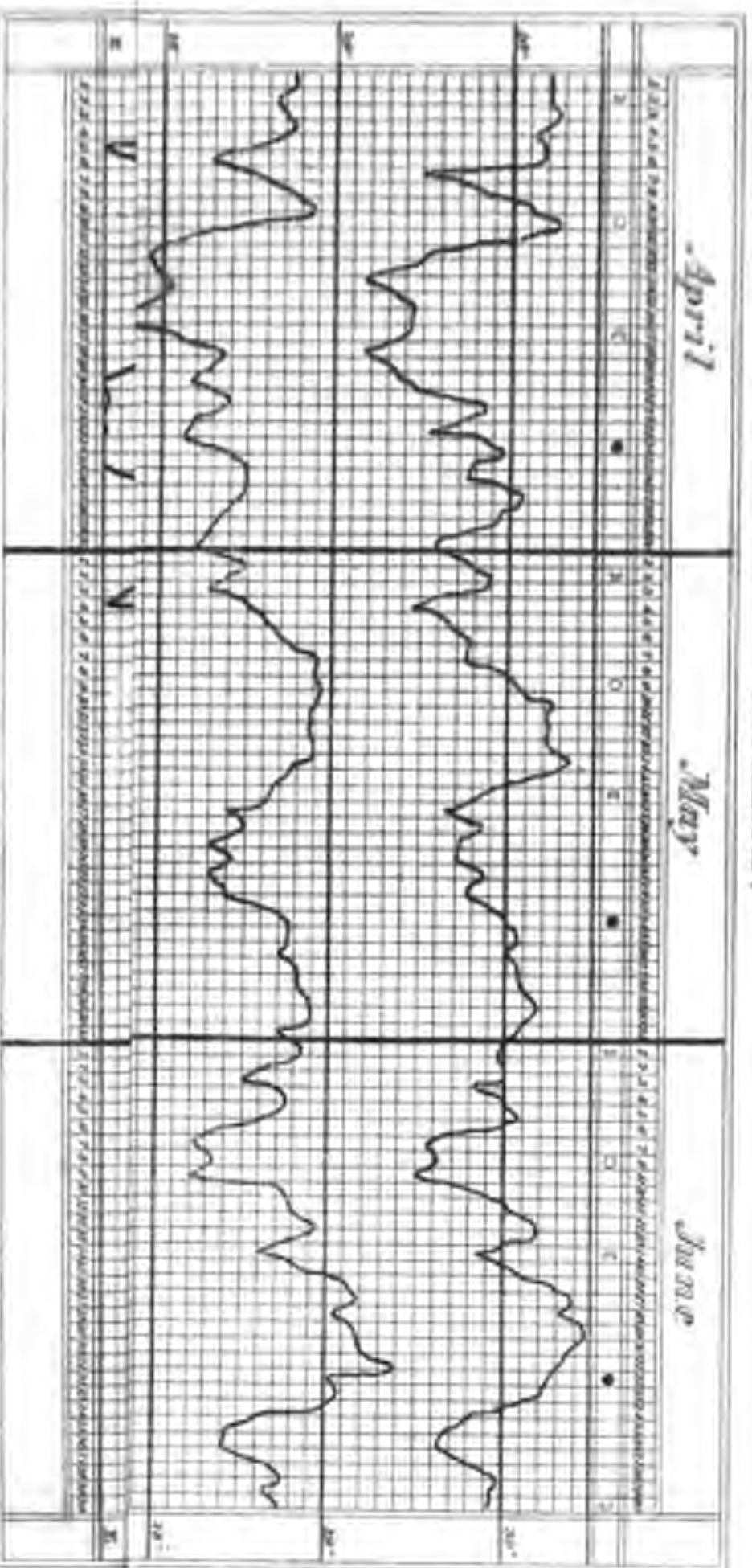


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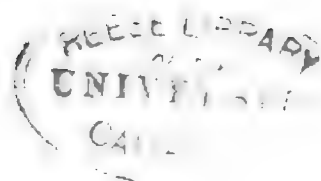




Fig. 1.



Fig. 6. a



Fig. 2.

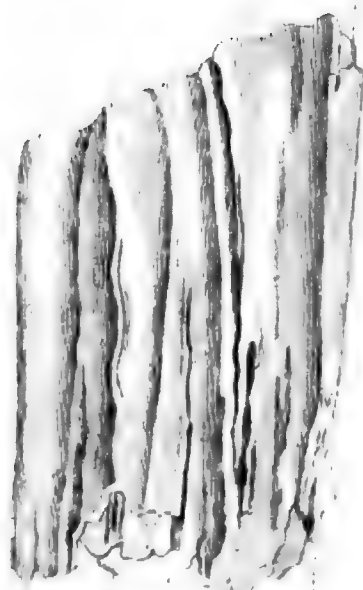


Fig. 5. a

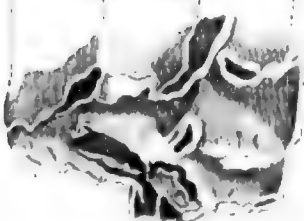
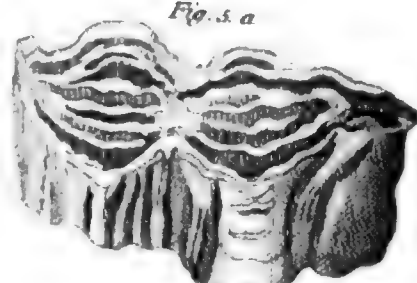


Fig. 4.



Fig. 3.



CHINESE

ANNALS  
OF  
PHILOSOPHY.

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JULY, 1819.

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ARTICLE I.

*Biographical Notice of the late Archibald Bruce, M.D. Professor of Materia Medica and Mineralogy in the Medical Institution of the State of New York, and Queen's College, New Jersey, and Member of various learned Societies in America and Europe.\**

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William Bruce (the father above-mentioned) and his brother Archibald, together with a sister, were natives of the town of Dumfries, in Scotland, where their father was many years resident as the parochial clergyman; and so continued until his decease, much respected.

Both sons applied themselves to the science of medicine and surgery. William, as above stated, became a physician in the

\* From Silliman's American Journal of Science, ol. i. p. 299.



British army, and died in that station of the yellow fever, in the island of Barbadoes ; and Archibald received a commission of surgeon in the British navy, in which he continued until disqualified by old age, when he retired from business, and died a few years since in London. For many years he acted as surgeon to the several ships commanded by Sir Peter Parker, Captain, and afterward Admiral.

Dr. William Bruce, before his final separation from his family, on the occasion of his being ordered to the West India station, had always declared that his son Archibald should never be educated for the medical profession ; and finally enjoined such instruction upon his wife and friends, to whom the charge of the boy was committed. After his decease, the same injunction was repeated by the uncle, then in Europe, who was ever averse to his nephew's making choice of this profession : much pains were, therefore, early exerted to divert him from such inclination.

The momentous state of political affairs induced his mother to send him to Halifax, under the care of William Almon, M.D. a particular friend of her husband, with whom, however, remaining but a short time, he returned to New York ; and was placed at a boarding-school at Flatbush, Long Island, under the direction of Peter Wilson, LL.D. who was in high standing as a teacher of the languages.

In 1791 he was admitted a student of the arts in Columbia College. Nicholas Romaine, M.D. was at this time among the physicians of highest consideration in New York, and was engaged in delivering lectures on different subjects of medical sciences in Columbia College. Having pursued the early part of his medical studies with Dr. William Bruce, he felt a generous gratitude for the instruction and attention which he had received from him, and endeavoured to requite them by advising with his son, and promoting his views, as far as lay in his power. Here commenced a friendship which increased with advancing years, and terminated but with life. At this period, young Bruce began to evince a desire to oppose the inclination of his father and friends by studying medicine ; this study, without their knowledge, and while a student of the arts in the senior class, he commenced by attending Dr. Romaine's lectures. Such was the strong bent of his mind towards the study of medicine and its collateral physical pursuits, that the persuasion and remonstrances of his friends proved alike ineffectual, and he soon gave free scope to the prevailing inclination.

The collection and examination of minerals, a pursuit not then at all attended to in this country, was his particular relief from other studies ; for even during his recreation, he was ever on the look-out for something new or instructing in mineralogy.

Dr. Romaine being about visiting Europe, young Bruce pursued his studies with Samuel Bard, M.D. ; and having attended the usual courses in Columbia College, he left the United States

for Europe in 1798, and in 1800 he obtained the degree of Doctor in Medicine from the University of Edinburgh, after defending a Thesis, *De Variola Vaccina*.

Having now finished his medical studies, he was prepared to visit the continent of Europe with peculiar advantage; for his continued attachment to mineralogy, a liberal distribution of American specimens, then comparatively new in Europe, and his social habits and dispositions, which were very conciliating, secured him the best introductions from Edinburgh, and laid the foundation of permanent friendships.

During a tour of two years, he visited France, Switzerland, and Italy; and collected a mineralogical cabinet of great value and extent. After his return to England, he married in London, and came out to New York in the autumn of 1803, to enter on the active duties of a practitioner of medicine.

Previous to the year 1805, the practice of physic in the state of New York was regulated by no public authority, and of course was not in the happiest condition to promote the respectability and usefulness of the profession. To remove as far as possible the existing inconveniences, Dr. Bruce became an active agent, and in conjunction with Dr. Romaine and other medical gentlemen of New York, succeeded in establishing the state and county medical societies, under the sanction of the state legislature. This act "may be considered among the first efforts made in this country to reduce medicine to a regular science, by investing the privileges of medical men in the body of the members of the profession."

In the organization of the College of Physicians and Surgeons of the state of New York, Dr. Bruce and Dr. Romaine were eminently active; and by their united exertion and perseverance (opposed by much professional talent) they obtained a charter from the regents. In this new institution, as professor of the *materia medica*, and of his favourite pursuit mineralogy, he exhibited the fruits of arduous study, with a dignity of character, and urbanity of manner, which commanded the respect of the profession, and the regard of the students.

The ruling passion in Dr. Bruce's mind was a love of natural science, and especially of mineralogy. Towards the study of this science, he produced in his own country a strong impulse, and he gave it no small degree of eclat. His cabinet, composed of very select and well characterized specimens; purchased by himself, or collected in his own pedestrian and other tours in Europe, or, in many instances, presented to him by distinguished mineralogists abroad; and both in its extent, and in relation to the then state of this country, very valuable, soon became an object of much attention. That of the late B. D. Perkins, which, at about the same time, had been formed by Mr. Perkins in Europe, and imported by him into this country, was also placed in New York; and both cabinets (for both were

freely shown to the curious, by their liberal and courteous proprietors) contributed more than any causes had ever done before, to excite in the public mind an active interest in the science of mineralogy.\*

Dr. Bruce, while abroad, had been personally and intimately conversant with the Hon. Mr. Greville, of Paddington Green, near London, a descendant of the noble house of Warwick, the possessor of one of the finest private cabinets in Europe, and a zealous cultivator of mineralogy. Count Bournon, one of those loyal French exiles, who found a home in England, during the storm of the French revolution, was almost domesticated at Mr. Greville's, and was hardly second to any man in mineralogical, and particularly in crystallographical knowledge. His connexions with men of science on the continent were of the first order, and to be familiar at Mr. Greville's, and with Count Bournon, was to have access to every thing connected with science in England and France. Dr. Bruce was also at home at Sir Joseph Banks's, the common resort of learned and illustrious men. Thus he enjoyed every advantage in England; and when he went to the continent, the abundant means of introduction which he possessed brought him into contact with the distinguished men of Paris, and of other cities which he visited. The learned and estimable Abbé Haüy was among his personal friends and correspondents; and many others might be mentioned in the same character, whose names are among the first in the ranks of science, in various countries of Europe.

Returned to his own country, after being so long familiar with the fine collections in natural history, and especially in mineralogy, in various countries in Europe, Dr. Bruce manifested a strong desire to aid in bringing to light the neglected mineral treasures of the United States. He soon became a focus of information on these subjects. Specimens were sent to him from many and distant parts of the country, both as donations, and for his opinion respecting their nature. In relation to mineralogy, he conversed, he corresponded extensively, both with Europe and America; he performed mineralogical tours; he kindly sought out and encouraged the young mineralogists of his own country, and often expressed a wish to see a journal of American mineralogy upon the plan of that of the School of Mines at Paris. This object, it is well known, he accomplished; and in 1810, published the first number of this work. Owing to extraneous causes, it was never carried beyond one volume; but it demonstrated the possibility of sustaining such a work in the United States, and will always be mentioned in the history of

\* The collection of Mr. Perkins became in 1807 (partly by the liberality of its possessor, and partly by purchase) the property of Yale College, and is now in the cabinet of that institution. It is believed that few cabinets of equal extent ever contained more instructive and beautiful specimens, with less that is unmeaning or superfluous. The cabinet of Dr. Bruce has, since his death, been purchased by a gentleman in New York for 5000 dollars.—Dr. SILLIMAN.

American science as the earliest original purely scientific journal of America.

Dr. Bruce had, in a high degree, the feelings of a man of science. He was ever forward to promote its interests, and both at home and abroad was considered as one of its most distinguished American friends.

Many strangers of distinction came introduced to him, and his urbanity and hospitality rarely left him without guests at his board. During the latter part of his life, he seems to have been less interested in science. His journal had been so long suspended, that it was considered as virtually relinquished; his health was undermined by repeated attacks of illness; and science and society had to lament his sudden departure, when he had scarcely attained the meridian of life.

He died in his native place on Feb. 22, 1818, of an apoplexy, in the 41st year of his age.

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## ARTICLE II.

*On Heat and Climate.\** By John Leslie, Esq. Professor of Mathematics in the University of Edinburgh.

THE word *heat* is of ambiguous import, and signifies either a certain sensation, or the external cause of that sensation. In this latter sense, considered as an attribute or accident of matter, heat forms an interesting subject of physical inquiry. Curiosity prompts us to consider in what its nature consists. Is it a state or condition of bodies, or is it a peculiar substance which, by its union with them, communicates certain properties? Some philosophers hold it to consist in the intestine motion of particles, and allege, in confirmation of their doctrine, that it is always excited by percussion. The chemists, on the other hand, have inclined to the supposition of an igneous fluid, and their opinion is now almost universally adopted. The former hypothesis indeed can hardly furnish any solid argument in its support. When a ball is struck, the tremor soon grows languid, and dies

\* This early performance was read at two several meetings of the Royal Society of London, as far back as February or March, 1793. It was not, however, admitted into the Transactions of that learned body, but retained and deposited in their archives. Fortunately I had preserved some notes, from which I was enabled soon after to complete the copy; and at the distance of twenty-six years, I am now induced to draw the paper from oblivion, and to communicate it to the public through the independent medium of a respectable philosophical journal. With all its imperfections, the essay will be found to contain not only the rudiments of my subsequent researches, but to open some original views which, even in the present state of science, must, unless I am greatly mistaken, be deemed new. It will besides ascertain the priority of certain discoveries with the history of which the public is unacquainted. I have, therefore, printed the text exactly as it stood, and only subjoined a few remarks and corrections.—AUTHOR.



away; and, for the same reason, if heat consisted in intestine motion, it would quickly disappear. But this conclusion is belied by fact. Heat in no case suffers any destruction; it is only transferred to other bodies, and gradually diffused through the general system; and could any substance be completely detached from other matter, there is the strongest reason to believe that it would for ever retain the same temperature. The communication of heat in rarified air becomes sensibly diminished; and in the imperfect vacuums which we are able to produce, the difference is very remarkable. It is highly probable, therefore, that if the air in which a body is immersed were completely abstracted, there could be no diffusion whatever of heat.

Language was formed to express human sentiment and feelings; and all the epithets which it employs refer to our own frame and constitution as the standard of comparison. Such are the terms *heat* and *cold*, which, though opposed to each other in common discourse, denote the impressions that may originate from the same cause, only varying in intensity. Had the vital functions been maintained at a much lower temperature, many objects at present denominated *cold* would have received the appellation of *hot*. If heat then be material, the term *cold* may be omitted altogether in philosophical discourse; and the various temperatures which substances exhibit may be ascribed to the different proportions which they contain of the igneous fluid. But the question occurs, what is this igneous fluid? Is it of a specific and unalterable nature, incapable of being invested with any other form? Or, since it evidently has some relation to *light*, is it a modification of this subtle matter? The former is the opinion generally received, but it is contrary to analogy. Every substance with which we are acquainted is susceptible of various aspects and combinations.

Heat, then, is manifestly allied to light. Is it a modification of that fluid, or is it not the same matter, only in a state of combination with other bodies? The latter hypothesis is recommended by its simplicity, which is the great object of philosophical research. Nature herself courts simplicity, and her boundless extent of operations result from the application of only a few general laws. That quality affords likewise a strong presumption of the truth of an hypothesis; for the difficulty of preserving simplicity increases in proportion to this quality itself of the structure, since the possible chances of combination are thus abridged. The arguments which may be brought to prove that heat is only light in a state of union with bodies are solid and conclusive. If a black body be exposed to the sun's rays, the incident light is no longer recognized by the senses; but it cannot suffer annihilation; it, therefore, unites with the body, and heat ensues. The rate too with which the heat accumulates is exactly proportional to the

intensity of the incident light. If the initial effect, for instance, of the sun's rays on the blackened bulb of a thermometer be diminished one-tenth by the interposition of a bit of plate-glass, that reduced quantity will also lose a tenth by the addition of another bit of the same glass; and thus, by continuing the application of the transparent plates, the changes produced upon the thermometer will form a descending geometrical progression. The same consequences will follow if the light, previous to its falling upon the bulb of the thermometer, be repeatedly reflected from mirrors. I find likewise that the intensity of the rays emitted from the sun at different altitudes as indicated by the thermometer, corresponds precisely with the quantities formerly assigned by the ingenious M. Bouguer from very different principles. If heat then accompanies invariably the absorption of light, and is proportional to the quantity absorbed, what more is requisite to establish their identity.

If the blackened bulb of a thermometer be inclosed by a slow conducting substance with a front of glass or mica, and held perpendicularly to a sun-beam, the mercury will rise at first with a regular and uniform motion. The celerity of ascent, however, soon begins to deviate from equability, and will by successive diminutions at last disappear. But it is easy to account for these anomalies. After the bulb of the thermometer is sensibly affected by the solar rays, the contiguous air also becomes warm by communication, and flows from that bulb towards the case or including substance; a circulation is thereby produced, and the rapidity with which the heat is conducted off is proportional to the increase of temperature in the bulb itself. While the accumulation of heat, therefore, is perfectly equable, its dissipation continually augments, till these opposite causes come to balance each other, and then the mercury will remain stationary at its extreme height. The initial change on the thermometer is in every case the only certain and accurate measure of the communication of heat.\*

The rays of the sun collected in the focus of a powerful burning lens or mirror are able to fuse, and even to volatilize, the metals, and the most refractory and opaque stones; yet they produce no remarkable effect on glass and pellucid crystals, and still less on water. These experiments evince that the heat is not *excited* by the impulse of the light, but is communicated merely by absorption. In the case of the opaque bodies, the influence of the incident beam being confined almost to the surface, the heat accumulates there with vast rapidity, which is not sensibly diminished by the subsequent transfusion through the internal mass or by dissipation in the air. A part only of the light which falls on pellucid substances is absorbed in its passage, and

\* I did not then suspect that I should afterwards invent so delicate an instrument as the *Photometer*.—A.

that quantity is greatly attenuated by the extent through which it is spread. The accumulation of heat, therefore, in a given portion of the mass, being on both accounts so slow, will soon be counterbalanced by the dissipation that ensues. Several causes concur to diminish the effect in water. The absorption of light is extremely small in that fluid, and the heated portions dilating rise continually to the surface and produce a general circulation; so that the heat is quickly diffused through the whole. If the water exposes a surface equal to that of the lens or mirror, it will be less heated than if set directly in the sun.

It has often been objected to the theory of the identity of light and heat, that the moon's rays collected in the focus of a powerful burning glass produce no sensible effect on the bulb of a thermometer. But if we consider that her light is 300,000 times weaker than that of the sun, we shall not be surprised that it has not been detected by the rude experiments hitherto made. I have no doubt, however, that the quantity might be ascertained, if an exceeding nice thermometer were provided, capable of bearing divisions to the  $\frac{1}{1000}$ th part of a degree, and the bulb inclosed within a glass vessel from which the air is exhausted as much as possible.

It has been urged by some, that light and heat are distinct fluids, but so related that the presence of the one occasions the extrication of the other, because the former passes freely through transparent substances, which the latter does not. Thus the image of the fire in the focus of a lens produces no alteration whatever on the thermometer, but in that of a concave mirror it has a very sensible effect, which, however, would be prevented by interposing a bit of glass. We must remark that the heat of the fire is derived from two causes; from the light emitted, and from the warm air which flows on all sides from the vicinity of the live coal. Bodies placed extremely near the fire are sensibly affected by the light only, as appears from an experiment of my worthy friend Mr. T. Wedgwood; but at a considerable distance the influence of the light may be totally disregarded. It is easy to see that a concave mirror will throw back the streams of heated air towards its focus, while a lens will completely stop their progress. For the same reason, hardness and polish are not necessary to the reflector, and a wooden bowl will produce the same effect. Even a slender netting composed of numerous meshes will prevent in a great measure the communication of heat by opposing a considerable resistance to the passage of the warm air; and, from the same principle, if a circular hole, not exceeding an inch in diameter, be cut in a fire-screen, a thermometer placed a little behind the aperture will scarcely be affected. If a hot body be placed in one of the conjugate foci of two concave mirrors, the air which flows from it upon the nearer mirror will be thrown towards the more remote, and thence reflected to

the other focus. The experiment will succeed though the mirrors have not a finished surface.\* It is only necessary that they be pretty near each other, that their concavities be suited to the position of their foci, and that the apartment be kept still and undisturbed. If a cold body be held in one of the foci, a thermometer will sink in the other; and this fact has been alleged as a proof, that cold, as well as heat, is a positive quality. But there is no occasion to have recourse to such an hypothesis, since the body will evidently be environed by a chill atmosphere, which will afford a constant ærial efflux. From these principles we may gather, that the conducting quality of air is not proportioned to its density; for though a greater number of particles come in contact with the heated body at one time, their succession is slower by reason of the diminished volubility of the fluid.

These general observations prepare us for a closer investigation of the nature, properties, and effects, of heat. I am aware of the difficulties to be encountered in the attempt; and of the temerity of searching into the constitution of matter. The magnificent spectacle of the celestial bodies invites our attention; and the application of the simple laws, to which their motions conform, affords the finest and most satisfactory exercise of the understanding. But in the elemental structure of the universe, nature seems wrapped in impenetrable secrecy. All is dark and forbidding, and to obtain even a glimpse of her abstruser operations would prove highly gratifying to a liberal mind.

It is an obvious remark that no substance in nature is ever permanent. Matter assumes an infinite variety of forms. Water changes into ice, and hail, and snow, as well as into invisible steam. The metals liquefy and rise into vapour; nor are the hardest rocks exempted from similar alterations in their constitution. Composition and dissolution form the whole of the science of chemistry. But the powers of nature are incomparably superior to the resources of art. Water and air alone are found in many cases to be sufficient for the growth and support of animals and vegetables. Yet how astonishing the variety of substances developed in plants? They are composed of charcoal, the primitive earths, several kinds of salts, gums, resins, oils; and, what is still more remarkable, they contain a portion of iron, manganese, and gold. The products of the animal kingdom are even more complicated, and thus the apparently simple substances, water and air, are, by a change of combinations, exhibited in an almost endless variety of forms. Nor can we hesitate to infer that any one substance may be converted into any other, though it may exceed our limited powers to produce the change.

\* It will now be perceived that some of these assertions are unguarded and inaccurate. But my attempts afterwards to reduce them to experimental proof led to most of the discoveries explained in the "*Inquiry into the Nature and Propagation of Heat.*"—A.



All matter is, therefore, essentially the same; and the sublime scene of the universe owes all its splendour and beauty merely to the variety of its composition. We behold a system of perpetual fluctuation. The materials remain indeed unaltered, but nature labours incessantly to demolish and to repair her stupendous fabric.

All bodies are compressible, and the only difference is, that some are more affected by the application of the same force than others. Were human power unlimited, there would likewise be no bounds to the condensation which we could produce. Hence we may infer that matter consists of particles placed at certain distances from each other. But bodies, whether compressed or dilated, endeavour to recover their constitution, and therefore their particles must occupy the limits between attraction and repulsion. Bodies are also susceptible of various constitutions, which proves that there are many quiescent or neutral positions.

It follows consequently, that the particles of matter are endued with certain attractive and repulsive powers, which run into each other, and vary with the distance according to some law. The action upon a remote object is attractive; and as, in this case, the exertion of all the particles may be reckoned equal, the joint effect will be proportional to their number. Hence it is that, whatever changes a body may undergo in its structure and particular properties, its weight will continue invariably the same. On the other hand, when the distance between the particles is extremely diminished, their mutual action must be repulsive, else the universe would in time be collected into a single point. If these elementary particles had any magnitude, their opposite sides would exert a prodigious repulsion against each other, and occasion a perpetual subdivision and dispersion. We are obliged therefore to admit, that they are only mathematical points, to which certain powers are directed. In short, the external world has a real existence, which yet consists in mere forces and *loci*.\* Such is the substance of the ingenious and profound theory of the late Abbé Boscovich. Some of the conclusions will perhaps be deemed paradoxical; but such is the fate of all our inquiries into the intimate essence of matter. And these difficulties arise from the superficial, inaccurate notions adopted in common life, which unfit us in a great measure for high abstraction. A mature reflection will convince us of the solidity as well as beauty of the system.

Whether the particles of light are the primæval points, or simple combinations of them, I shall not venture to decide. It appears sufficiently from the experiments on inflection, and those with thin plates, that, in approaching other substances,

\* A physical particle is only a cluster of primæval points, whose attractions and repulsions, sometimes conspiring, and sometimes counteracting each other, will form a compound action varying extremely according to the figure of the arrangement. Hence the prodigious diversity in the properties of bodies.

their repulsions and attractions repeatedly fluctuate. But at exceedingly small distances, the action of light is uniformly attractive, as appears from its refraction. If the luminous particles in their passage through a body be acted upon equally on every side, their motion will not be affected: on the contrary, if they happen to encroach within the limit due to any cluster of points, their progress will, according to the degree of its proximity, be either deranged or totally stopped. Hence the dispersion of light observed both in mirrors and lenses, and hence too the diminution of its intensity occasioned by absorption. The distinction of bodies into transparent and opaque is not founded in nature. As no substance is perfectly transparent, so none is perfectly opaque; and the extremes are connected by an extensive intermediate series. In the case of exceedingly thin plates, we are obliged to admit the truth of the general proposition; but, in thick masses, the quantity of light transmitted is so extremely minute as to make no sensible impression on our organ of sight. In short, bodies approach to opacity, in proportion to their density, and the irregularity of their constitution.

Whenever a body has a redundant quantity of heat, it makes a copious emission of light; which implies that there subsists between the particles of this fluid a certain repulsion, proportional most probably to the reciprocal of their distance. Such at least is the conclusion into which we are drawn by the analogy of the æriform fluids. In these, however, the sphere of repulsion has some palpable extent, and the joint actions of all the included particles constitute the elasticity, which must be proportional to the number of similar forces, or to the density. But in the case of light, the repulsion most probably is confined to the adjacent particles: for the plane surface of a luminous body emits rays equally in every direction,\* nor is there any alteration in this respect occasioned by giving a high polish; a certain proof that the limit of the repulsion between the particles of light is incomparably nearer than that at which they are reflected from a mirror. A similar inference might be drawn from the perfection of optical instruments, which could not have been the case if the repulsion of the rays had any considerable extent. The particles of light, however, must be vastly more distant from each other than those of other matter, since no increase of weight has ever been observed to accompany the addition of heat.

The particles of light combined with a body endeavour to distend, but are confined by the attraction of the other matter. Upon every accession of heat, the particles of light must approach towards each other, and towards those of the body; both the repulsions, therefore, and the attractions, will receive an augmentation, attended with a general expansion. If the increase of the attraction of the luminous particles to the ad-

\* I have since proved that the intensity of the light or heat emitted from any surface is proportional to the sine of the inclination.—A.

jacent matter arising from approximation observed the same proportion with their repulsion, a body would be capable of imbibing any quantity of heat, without emitting it in the form of light. But after a certain accumulation of heat, the balance is destroyed; and as nature admits only gentle transitions, we may reasonably conclude, that the attractive power increases regularly at a slower rate than the repulsive. The attraction of the particles of matter to each other, which is the third force necessary to the general quiescence, appears in all ordinary cases to be exactly proportional to the quantity of dilation: for a solid body yields the same musical note with whatever violence it is struck; this is likewise true of air, and other similar fluids, and is the principle of wind instruments; and the experiments performed in the receiver of an air-pump evince that the expansion of water and other fluids corresponds precisely to the pressure removed. The distention of a body being the measure of the mutual attraction of the particles of matter, is a certain proof that the combined light is in a condensed state. But this distention will not be the same in all bodies, since the attraction occasioned by a certain derangement differs in each. Even, in the same body, equal additions of heat applied successively will not produce uniform expansions, which must have been the case if the attraction of the luminous particles to matter increased at the same rate with their mutual repulsion; but as the latter force begins to exceed the former, it occasions a further dilatation. We may, therefore, state it as a general principle, that the expansions produced by equal accessions of heat, form a rising progression. Mercury discovers this property; spirits of wine indicate it in a more remarkable degree; and the expansions of water between the freezing and boiling points correspond to the series of square numbers. It is true that equal increments in the longitudinal dimensions would be attended with progressive augmentations of volume: thus the differences of the cubes of 10, 11, 12, 13, &c. are the numbers 331, 397, 469, &c. But this consideration is insufficient for the great anomalies observed. The above principle will explain other facts that appear to have no immediate dependance on it. The contraction, for instance, which a given pressure produces on cold water is greater than that on warm; inasmuch that the effects at the temperatures of  $45^{\circ}$  and  $65^{\circ}$ , I find to be nearly in the proportion of 4 to 3. The reason seems to be this:—In the accumulate state of the igneous fluid, a certain condensation occasions a considerable excess of the repulsive above the attractive powers, and thence arises a dilatation which diminishes the equal contraction that otherwise would take place.

Our views lead also to another curious inference, namely, that a body which emits light copiously is in its state of the utmost distention. Hence this emission will not be conjoined with the same temperature in all bodies; the more dilatable

fluids in particular will admit of being heated to a very high degree, which corresponds with experiment. Every substance most probably contains the due mixture of the coloured particles of light, but the attraction of the more refrangible seems to deviate sooner from the mutual repulsion. It is thus that, in the progress of heating, bodies in general assume successively the tints of red, orange, yellow, and white; the emission consisting at first of the most refrangible rays, and afterwards including gradually a mixture of the others.

If two portions of a body be heated unequally, the luminous particles on the confines will flow from the redundant fluid by the operation of two causes; the excess of the mutual repulsion on the one hand, and the excess of the attraction of the cold matter, arising from the greater density on the other. But the celerity with which the equilibrium is restored must depend on a variety of peculiar circumstances; such as the difference of temperature, the extent of the body, and the figure, arrangement, and interval of its integrant particles. In fluids the diffusion of heat is in general quicker and more uniform; because the irregular density of the mass occasions an intestine motion. If a part of a body be compressed, the contained particles of light will approach each other, and likewise the rest of the matter. But the attraction, as we have seen, increases at a slower rate than the repulsion; wherefore some of the light will pass into the other part of the body, which is in the natural state. This principle affords a satisfactory explanation of the heat extricated by percussion and friction. Thus, when I strike two stones against each other, a certain condensation is produced in both at the points of collision, and heat flows into the interior mass. But, recoiling like a spring, they presently acquire dilatation equal to their former compression; the parts struck are now disposed to imbibe more heat than at first, and this is supplied from all the contiguous matter, especially from the air. By repeated percussion, the stones in this manner receive continual accessions of heat, which, though separately small, amount by their number to a very considerable quantity. In the case of friction, the pressure is successively applied to different points of the surface, so that the sources of heat are multiplied. The same explanation may be extended to all hard bodies. Even fluids may acquire some heat if their surface be violently agitated; and hence the foundation of the ancient remark, that the sea is sensibly warmer after the fever of a storm.\*

But there are many substances capable of receiving a per-

\* This explication of the heat occasioned by friction is subtle, and could not easily be brought to the test of experiment. If the sea be generally warmer after a storm, this must be owing to the heat then communicated to the superficial water from the atmosphere, which has regained its temperature, after the depression, during rain.—A.



manent condensation. Clay, for instance, is brought into that state by burning, and the metals by hammering. By this change of constitution, these bodies are rendered incapable of retaining their natural share of heat. The particles of this fluid have their attraction to the matter increased, but not in an equal degree to that of their mutual repulsion. The relative proportion of these forces, which may be called the *specific attraction* for heat, will, therefore, determine the quantities of that fluid contained in equal communicating masses; and an equilibrium of temperature will obtain whenever the quantities are as the specific attractions. The term *capacity* has been employed to express the same property of bodies, but the idea which it naturally suggests is perhaps inadequate.

This train of speculations leads to a curious and important fact, which is, that, if substances be ranged according to their densities, their specific attractions for heat will in general follow the inverted order. Thus when the temperatures are alike, hydrogenous gas contains more heat than atmospheric air, and both these fluids more than water\*; the quantity is still smaller in the earths and stones, and very minute in the metals: even among these the same principle may nearly be traced; there is a greater specific attraction in iron than in tin, in tin than in lead, in lead than in mercury, and the oxides bear a similar relation to their metals. There are a few exceptions, however, to the general principle; ice, for instance, has a smaller specific attraction than water, and mercury than gold. Nor is this to be wondered at; for though the condensation of matter tends to diminish that force, yet, as it results from the joint action of all the elementary points, it must likewise be affected by a change in their arrangement and constitution. If the peculiar properties of a substance be not altered, the general principle will invariably apply. Hence, the specific attraction of any body for heat, must perpetually fluctuate according to the degree of dilatation arising from whatever cause. This fluctuation is in most cases inconsiderable, yet appears to be one of the chief sources of error in the solution of the curious problem of finding the thermometric zero.†

No substance more easily receives a change of volume than air; and its specific attraction for heat may, therefore, be presumed to be liable to great alterations. But to determine these with accuracy is a very difficult investigation.

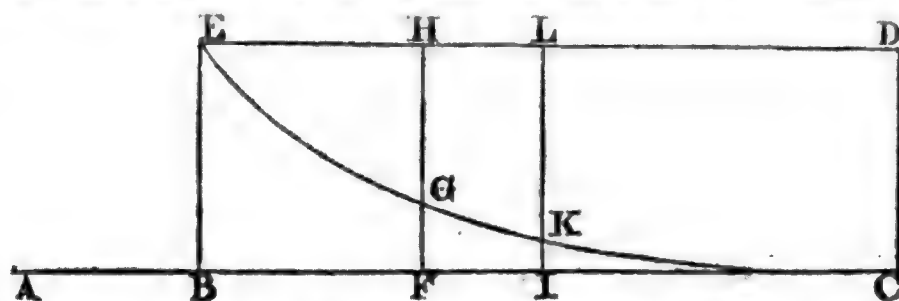
The most obvious plan would be this: suspend in the receiver

\* Such was the opinion which then prevailed, Dr. Crawford having reckoned the capacity of air near double that of water. But my hygrometrical researches, some years afterwards, convinced me that it is six times less, not exceeding three-tenth parts of the capacity of water.—A.

† We may remark by the way that the principles already investigated show that light is emitted with equal velocity from all luminous bodies; a fact which theory might deduce from the common observation, that the rays of the same species, from whatever object they are transmitted, suffer equal refractions on en-

of an air-pump a fine loose bag of silk, including the bulb of a delicate thermometer. When the air contained within the receiver is partly exhausted, the bag will become distended, and, after some time, its rarefied air will attain the temperature of the room. If the cock be now opened, the bag will collapse, and the included air, suffering a diminution of its attraction for heat, will indicate a high temperature. But before the external air is completely admitted, or the thermometer has acquired its just temperature, a great part of the heat extricated is lost on the bag, or conducted off through its substance. As these circumstances are accidental and irregular, the rise of the thermometer will indicate not the exact change of the air's temperature, nor even the proportional change. I had, therefore, recourse to a different method for the solution of this interesting problem. I reflected that, if warm air be admitted into a glass vessel, the internal surface will almost instantly be heated, and the air cooled down to the same standard. The subsequent cooling is extremely slow, for the rate at which the heat is communicated is inversely as the depth it has penetrated into the glass, which is besides a very bad conductor. But as the surface of the vessel and its contents are constant, the excess of temperature retained by the air after admission will in every case bear the same ratio to what it possessed previously. I suspended an exceedingly delicate thermometer in a large receiver 900 cubic inches in capacity, and extracted one-fifth of the contained air: after the general temperature was exactly diffused, I suddenly admitted the external air, and observed the mercury of the ther-

tering the same medium, and consequently produce perfect vision. As during the emission of light from a body each particle is succeeded by another, it will be exposed during the imperceptible interval of space to the general repulsion of the combined luminous fluid. Let A be situated on the surface of the body, and B at



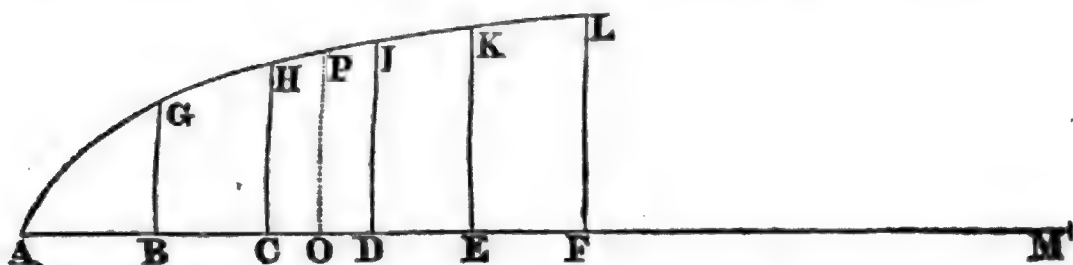
that of the fluid, which will protrude a little farther; let BC denote the mutual distance between the particles of the fluid, BE the general repulsion, and the ordinates GF, IK the attractions of the body at the distances AF, AD, and suppose the curve to coincide with the straight line AC, which appears very probable. Then, if by any cause, the attraction BE is in the smallest degree diminished, or the repulsion BE increased, the particles will be propelled, and will afterwards be carried forwards by the excesses GH, and LK of the repulsive above the attractive forces. Hence, from the principles of dynamics, the square of the final velocity will be as the curve space EDC K G.

But the curve is given in species, and consequently proportional to the circumscribing rectangle BE DC, which is given in magnitude, since the repulsion BE is inversely as the distance of the particles of the fluid BC. Wherefore the final velocity will in every case be the same.

momometer to rise with great rapidity  $3.1^{\circ}$ , where it remained stationary some seconds, and then very slowly subsided. The same experiment was performed on air three-fifths, two-fifths, and one-fifth of the common density, and the excess of temperature was found to be  $5.6^{\circ}$ ,  $7.7^{\circ}$ , and  $9.4^{\circ}$ . After the air was much exhausted, the quantity of rise hardly varied at all, so that it was easy to fix the extreme point at  $10.7^{\circ}$ . All these experiments were repeated at least twice with scrupulous attention. The above numbers do not, however, express the proportional increase of temperature of the rarefied air. The excess is not  $3.1^{\circ}$ , for example, in air of four-fifths the usual density, but  $\frac{4}{5} \times 3.1^{\circ} = 3.9^{\circ}$ ; because the heat contained in four parts of air is diffused through five. In the same manner the computation may be made for the other cases. The annexed table exhibits the general results :

Density.	Rise of Temperature.
Four-fifths .....	$3.9^{\circ}$
Three-fifths .....	$9.3$
Two-fifths .....	$19.7$
One-fifth .....	$37.0$
Vacuum .....	Infinite

The intermediate quantities might be found by interpolation; but it will be more convenient to obtain a general formula. For this purpose we shall recur to the former numbers. Divide the straight line A F into five equal parts in the points B, C, D, and E, erect the perpendiculars B G, C H, D I, E K, and F L, equal to  $3.1^{\circ}$ ,  $5.6^{\circ}$ ,  $7.7^{\circ}$ ,  $9.4^{\circ}$ , and  $10.7^{\circ}$ , and trace a curve through their extremities. Then if A F



denotes the ordinary density, and O F any other density, the ordinate O P will express the number corresponding to the latter. But the differences between the equidistant ordinates are  $2.5^{\circ}$ ,  $2.1^{\circ}$ ,  $1.7^{\circ}$ , and  $1.3^{\circ}$ , forming a descending arithmetical progression, of which the last term is half the first. Hence, producing the absciss till A F = F M, these differences will be as the distances from M. Put A O =  $x$ , and O P =  $y$ , and suppose these finite differences to be proportional to the fluxions of the ordinates, which will be very nearly true. Then  $y \doteq (2 - x) \dot{x}$ , and  $y \doteq 2x - \frac{1}{2}x^2$ . But when  $y = 1$ ,  $x = 10.7^{\circ}$ ;

whence  $y = (2x - \frac{1}{4}x^2) \frac{10.7^\circ}{1.5} = (x - \frac{1}{4}x^2) 14.27^\circ$ . This quantity must be augmented in the ratio of A F to O F, in order to find the proportional excess of temperature, which is, therefore,  
 $= 14.27 \left( \frac{x - \frac{1}{4}x^2}{1 - x} \right)$ .

To ascertain the actual quantities, which is a most important point, I took another glass receiver, whose capacity was one-third of that of the former, and its surface  $\frac{17}{8}$ ths, allowing for that of the circular pieces of leather on which it stood. The utmost rise of the included thermometer was observed to approach to  $7.5^\circ$ , and the other ascents were proportional to the numbers already discovered. But the quantity of heat lost on the glass will depend on the extent of surface compared with the capacity of the receiver. In the present instance, the effects would have been the same in receivers of equal capacity, but whose surfaces were as 27 to 40. The heat communicated to the surfaces is, therefore, as  $27 \times 10.7$  to  $40 \times 7.5$ , or as 288.9 to 300. Hence as  $300 - 288.9$  is to 288.9, so is the difference between the heat lost on the glass in the two cases, or  $10.7^\circ - 7.5^\circ$ , to  $83.3^\circ$ ; the heat lost on the smaller surface, or on the large receiver. Whence the whole heat extricated was  $94^\circ$ . Augmenting, therefore, the preceding formula in the ratio of  $94^\circ$  to  $10.7^\circ$ , we obtain  $125x \left( \frac{1 - \frac{1}{4}x}{1 - x} \right)$  for the decrease of temperature arising from a diminution  $x$  of the air's density. If  $x$  be made negative,  $125x \left( \frac{1 + \frac{1}{4}x}{1 + x} \right)$  will denote the augmentation of temperature due to an increase  $x$  of density.\*

This last formula affords a satisfactory explanation of several curious phenomena. If I blow with a common bellows against the bulb of a thermometer, the mercury will rise three or four degrees; because the air striking forcibly is suddenly condensed and its temperature elevated. But the effect must be precisely the same whether a body is exposed to a stream of air, or is carried with equal celerity through the still atmosphere. To verify this position, I took an hollow brass ball about an inch and half in diameter, and filled the cavity with mercury, having previously rubbed the inside with oil to prevent the action of this metal. I then fastened a string of a convenient length to the ball, and

\* I have since carefully repeated this experiment on a much larger scale, and with an air-pump of the best construction. The results are somewhat different and much simpler. If  $d$  express the density of air, while that of the common standard is denoted by unit, the corresponding difference of temperature is  $45^\circ \left( \frac{d}{1} - d \right)$

on Fahrenheit's scale; or putting  $x = 1 - d$ , the formula becomes  $45^\circ x \left( \frac{2 - x}{1 - x} \right)$

in which, chiefly, the coefficient is altered. See the article CLIMATE in the Supplement to the Encyclopædia Britannica.—A.



whirled it some minutes about my head. In one experiment, the contained mercury was heated to  $2\frac{1}{4}^{\circ}$ , and in another  $3^{\circ}$ . The ancients were well acquainted with this fact, but exaggerated it so much, that the moderns have treated the whole as a fiction. The Babylonians are said by Suidas \* to have roasted eggs by whirling them in slings; and Virgil, with the licence of a poet, represents a leaden ball as melted by the violence of the throw:

Et media adversi liquefacto tempora plumbo,  
Diffidit, ac multâ porrectum extendit arenâ.

Æn. ix. 588.

The work of Lucretius is didactic, and may, therefore, be presumed to adhere closer to truth; yet this author attempts to explain the origin of thunder from the principle alluded to, which he cites as a fact well known and established:

Mobilitate sua fervescit; ut omnia motu  
Perculefacta vides ardescere ———

But unfortunately he is led by a hasty analogy into a false conclusion:

————— plumbea vero  
Glans etiam longo cursu volvenda liquescit.

Liv. vi. 179.

This last clause suggests the way in which this curious consequence was drawn by the ancients. The slingers usually threw leaden bullets, which, being sometimes picked up immediately in order to be returned by their antagonists, would give a sensation of heat. Perhaps an opinion which, in a certain degree, is true, that the heat acquired is proportional to the length of the track, was the source of the hyperboles which cloud the subject.

But to estimate the precise effects, it will be necessary to bestow a closer attention. The density of the air at the surface of the earth is on an average equal to what would be produced by the pressure of a column of air 28,000 feet high, and of the same uniform density. Hence it may be computed that the compression of a whole atmosphere would project the aerial particles with the velocity of 1340 feet in a second. In other cases, the velocity of the stream will be in the subduplicate ratio of the difference of density. By reversing the supposition, we may conclude that when a flat body is carried directly through the atmosphere with the celerity of 500 feet in a second, the stratum of air on its anterior surface will suffer a condensation equal to  $\left(\frac{500}{1340}\right)^2$  or  $\cdot 1391$ . Consequently the temperature of this portion of the fluid must be augmented by  $125^{\circ} \times \cdot 1391 \left(\frac{1\cdot0348}{1\cdot1391}\right)$ , or about  $16^{\circ}$ . The velocity just stated may be produced by an

\* *Voxe Περδωρῶντες.*

expert slinger. If larger numbers be taken, such as 1000, 1500, and 2000 feet a second, the corresponding condensations of the air on the flat surface of the body will be found to be .5564, 1.2520, and 2.2258, and the augmentations of temperature  $51^{\circ}$ ,  $91^{\circ}$ , and  $135^{\circ}$ . The celerity communicated by fire-arms is generally 1,500 feet per second, sometimes 2000.

These deductions apply accurately in the case of thin cylinders; but the effects must be somewhat diminished with globes, since the air being displaced by an oblique stroke will not suffer so much condensation. Were the discussion of sufficient importance, an approximation might be found for the precise change of temperature. But an objection occurs that appears to have some weight. Is not the rarefaction of the air on the posterior surface of the ball equal to the condensation on the anterior? And if so, will not the cold produced on the one hemisphere compensate for the heat extricated on the other? I answer that the air behind follows the motion of the body; while that before is incessantly succeeded by other portions; so that the heating is permanent, and the cooling is only momentary. The rotatory motion common to projectiles will not in the least affect the operation of these causes. We must not, however, suppose that the heat will receive perpetual additions; there will be a certain standard of temperature corresponding to the velocity of the body, and which it can never surpass. This extreme point will be sooner attained by small balls than by large ones, because the former have a greater surface in proportion to their quantity of matter. The huge masses hurled from the mouth of a cannon may not acquire the just increase of temperature in the whole of their track.

I shall now proceed to investigate the diversity of temperature which obtains at different latitudes and at different heights of the atmosphere. I am sensible that on the first of these inquiries, my speculations are very imperfect; but I shall at least develope the principles which ought to enter into the computation. Were the theory of the motion of fluids tolerably complete, the absolute quantities might be assigned. However, so vast is the intricacy of this department of science, that I despair of ever seeing it arrive at such perfection.

The theory of central fire has been consigned to oblivion, and the heat of the earth ascribed wholly to the rays received from the sun. We might thence infer that our globe is growing continually warmer. There are data even from which the quantity of this increase of temperature can be determined. The mean density of the earth is above four times that of water, as my friend Dr. Hutton has deduced by ingenious and elaborate calculations from the observations made on the mountain Shehallien. This great density affords a strong presumption that the globe which we inhabit is composed principally of some metallic oxide, most probably that of iron. But the rust of iron is three

times lighter than mercury, and has only one-eighth of its specific attraction for heat. Hence the changes of temperature which equal additions of heat will produce on mercury and the ferruginous oxide are as eight to three. I exposed to the rays of the sun when at the altitude of  $60^\circ$  the blackened bulb of a mercurial thermometer with a very large scale, and observed the mercury to rise at the rate of a degree in 14 seconds. If the rays had reached the bulb without being impaired in their passage through the atmosphere, the thermometer would have risen a degree in 11". Wherefore had an equal ball of the rust of iron been substituted, the same effect would require  $\frac{1}{3} \times 11''$ , or half a minute for its production. But the quantities of light which spheres receive in like exposures are proportional to their surfaces, and consequently the changes produced in their temperature will be reciprocally as their diameters. The diameter of the earth, allowing for the atmosphere, may be estimated in round numbers at 8000 miles, and that of the bulb of the thermometer was six-tenths of an inch. Wherefore the time elapsed before the temperature of the earth increases one degree, must be  $\frac{12}{6} \times 5280 \times 8000$  or 844,800,000 half minutes, which amounts to 803 years and 238 days. The result will be the same whether the particles of light reach the surface, or are spent among the vapours and clouds of the atmosphere, as they will ultimately be communicated to the mass of the globe. If we reckon the distance of the absolute zero of the thermometric scale to be 2000 degrees, and suppose the earth to have received all its heat from the sun, we shall find that the enormous period of 1,600,000 years has elapsed since the primeval chaos. But the earth consists of materials of such a slow conducting quality that probably the heat is not yet equally diffused. No difference of temperature indeed is perceived in descending our deepest mines. These perforations, however, reach not beyond the external crust; far below them the cold may commence; and perhaps the genial influence of the sun has never yet penetrated to the centre. At any rate, we may reasonably presume that the superficial parts of the globe will be heated quicker than the vast internal mass. The increase of temperature may, therefore, exceed a degree in eight centuries; and as the same principles reduce the distance of the thermometric zero, they will likewise diminish somewhat the period we have assigned for the antiquity of the primeval chaos.\*

These views will, I am afraid, be treated by some as chimerical. What relates to the age of the world, I freely give up; but the more I reflect on the proposition, that climates are growing gradually warmer, the more am I convinced of its reality. The relations of the historians and the descriptions of the poets of

\* More correct calculations, on this subject, will be found in the "Experimental Inquiry into the Nature and Propagation of Heat."—A.

antiquity abundantly testify, that through the whole of Europe, and in some parts of Asia, the winters were formerly much severer than in the present times. I might cite many remarkable passages to this purpose, were I not apprehensive of extending this paper to an unusual length. Some late writers have indeed admitted the fact, but have ascribed it to the clearing of the forests, the draining of the marshes, and the general amelioration of the globe. But these causes evidently will not apply to Italy. That delightful country is divided on all sides from the rest of the world by the sea and by ridges of lofty mountains. Its ancient condition was flourishing and vigorous; its modern is sunk into insignificance and abject superstition. When it formed the seat of temporal empire, the wealth which flowed in from the provinces was mostly spent by the opulent Romans on the cultivation and improvement of the soil, and on the decoration of rural scenery, of which they were passionately fond. Besides, we shall presently be convinced that the clearing of a country has not the smallest effect in promoting the general warmth of the climate. The true tendency of such a change on the surface is to diminish somewhat the inequalities of the seasons; and this fact may serve to reconcile the apparently contradictory accounts transmitted from the ancients. The summers were formerly warmer upon the whole than at present, though the winters were much colder. Vineyards were cultivated in Britain by the Roman colonies. It is sufficient for the perfection of plants that they enjoy the due heat during the season of growth. In Virginia the winters are intensely cold; yet that country yields spontaneously many of the fruits of a tropical climate. For the same reason some exotic plants are raised in open air near St. Petersburg which require the assistance of artificial heat in the vicinity of London.\*

If the earth had not been clothed with an atmosphere, an immense diversity of climate would have obtained. The quantities of light received from the sun during the revolution of a year at the Equator, at London, and at the Pole, may be calculated to be as the numbers 31, 23, and 12; and consequently if the action of the sun's rays were confined to the surface, the parallel of London must have been colder than the Equator by  $516^{\circ}$ , and the Polar regions by  $1,226^{\circ}$ . In the long succession of ages, however, the portions of heat received in different latitudes would gradually penetrate toward the centre, and mingling in their progression, would, in some measure, temper the system. This consideration might somewhat diminish the above quantities, yet the diversity of climate would still be prodigiously

\* If experiments on the temperature of springs or wells were carefully made and recorded, it would gratify our latest posterity, who might thence incontrovertibly decide whether the heat of the globe be progressive. The common registers of the weather are of little use in a philosophical point of view. The height of the thermometer, being marked only at certain hours of the day, can never give the true average heat of the climate.



greater than what is actually observed. Upon such a supposition, the regions of the north would have been for ever divided from those of the south, and each parallel of latitude inhabited by a particular race of beings. Among the many beneficial purposes to which the element we breathe is subservient, this undoubtedly is one of the most important,—that it cherishes the polar regions by heat conveyed from the tropics, which, in return, it refreshes by northern gales; thus labouring assiduously to maintain a balance of temperature on the surface of the globe. Such a view of the subject corroborates the general presumption, that the planets and their satellites have all of them atmospheres.

It is a theory very generally received at present, that the solar heat is first evolved at the surface, from which it again rises, and dissipates itself through the atmosphere. Conjectures have been formed on the law of this ascent, and conclusions thence drawn concerning the rate with which the temperature diminishes at different altitudes. But were the principle accurate, those countries should be hottest which enjoy most abundantly a vigorous sun-shine. Our own island is confessedly warmer, on the whole, than the same parallels on the Continent; yet these regions are generally blessed with a clear and joyous heaven, while our “weeping sky” is shrouded by far the greater part of the year. Besides, the rays of the sun act most fiercely on the tops of mountains, where it surely is not warmer than in the valleys below. But the whole theory is founded on an erroneous assumption, which ought long since to have been banished from science: I mean the proposition, that light is transmitted through a transparent medium without any obstruction whatever. On the contrary, near one half of all the light which falls at the Equator is absorbed in its passage through the atmosphere, and, in the high latitudes, a much larger proportion. Inquirers have commonly committed another oversight, in supposing that air conducts heat like a solid body. But were that fluid completely confined, its conducting quality would be found incomparably inferior even to that of glass. In fact, that share of the heat which is communicated by the slow pervading of the aerial mass, may be totally neglected in every computation. It is by its motion alone that the air transfers and disseminates warmth; \* and for this purpose it is admirably fitted by its extreme volubility, and by the great variations produced on its density by changes of temperature. If that fluid had possessed these properties in a much higher degree, an almost uniform warmth would have prevailed over the earth. The wisdom of the Great Geometer † of the universe is displayed in the choice of the exact proportions, which, while they temper the system, preserve the variety of climate, and the grateful vicissitude of season.

\* I need scarcely observe that I was not yet acquainted with the pulsatory energy which belongs to the gaseous fluids.—A.

† I allude to the famous expression of Plato, *Θεὸς ἀνὴρ γυμνὸς καὶ ὀπίσσω*.

It is an undoubted principle, that there is a continual communication between the hot and cold portions of a fluid ; and, therefore, without appealing to actual observation, we may strictly infer a regular and perpetual circulation of air between the Equator and the Poles. No branch of physics is, indeed, so intricate or so imperfect as the doctrine of winds. We cannot judge of their direction with certainty from that which obtains near the surface ; since it appears from the relations of aëronauts that, in the higher regions of the atmosphere, various and even opposite currents exist at the same time ; the only discovery, perhaps, if it deserve that name, which has been made by balloons. These irregular streams of air result from local and accidental circumstances ; nor is there any reason to doubt but the general sum of motion is directed between the Equator and the Poles. The difference between the average temperature of different latitudes is such as to produce the exact celerity of communication necessary for maintaining perpetually the balance. The quantity of heat received from the sun, in the space of a year, is annually distributed over the face of the globe. Each place too must receive the same gradual increase of temperature, otherwise the rate of aërial circulation would in time be affected, and consequently the heat differently distributed, which would again restore the equilibrium. It appears, likewise, that the average heat of places, on the level of the ocean, must depend on their latitude. The formula which the celebrated astronomer, Professor Mayer, has given for computing that average, answers remarkably well. It is  $84^{\circ} - 52^{\circ} \phi^*$ ,  $\phi$  denoting the sine of the latitude. If the theory of aërial motions were perfected, a more accurate expression could probably be discovered from the principles we have been establishing.— America forms a curious exception to the general rule. Some philosophers have ascribed the unusual cold which prevails on that continent to the immense natural forests, which exclude the sun's rays, and expose a great surface to evaporation.\* The first of these causes should be set aside ; nor can any stress be laid upon the second, till it be proved that the quantity of humidity evaporated in the New World exceeds what is again precipitated in clouds and vapours. I am disposed to assign a different reason. Owing to some causes, which I will not pretend to determine, the prevailing wind in the United States of America is the north-west, which blows from the high bleak territory that extends from the Lakes of Canada to the inhospitable shores of the Pacific Ocean. It is prevented, by the tall ancient forests, from sweeping the surface ; and it, therefore, arrives on the settlements near the coast full charged with cold. Similar effects are probably produced in South America.†

\* See particularly an ingenious note, by my learned friend, Professor Robison, in Dr. Robertson's History of America.

† This explication, though conformable to the ordinary principles, is really not admissible. See the Article CLIMATE, already referred to.—A.

Heat is circulated with augmented rapidity in summer and in winter, a circumstance which, in some measure, tempers the inequality of the seasons. The quantities of light which fall in the high latitudes during the long summer days is surprisingly great. At the time of the æstival solstice, the luminous matter received in the course of a diurnal revolution at the pole exceeds that received at the equator in the proportion of 15 to 11. We might, therefore, expect the summer to be excessively hot in the polar regions; and this would probably be the case, were not the surplus heat expended in thawing the surface of an immense continent of ice. In Siberia and Lapland, the action of the sunbeams is sufficient, during the spring and part of the summer, to dissolve completely the snow and ice; and nature, released from her chains, hastens to pour forth, in gay luxuriance, the few vegetable tribes that had withstood the severity of the winter. The heat often grows oppressive, and engenders myriads of winged insects, which oblige the hardy natives to retire into their hovels and defend themselves with smoke. It is the vicinity of the ocean that prevents the same violent heats and rapid growth from being remarked on the northern extremity of our own island. A body of water receives and disperses heat infinitely quicker than a mass of earth.\*

But besides the great circulation of differently heated air between the poles and the equator, there is in every place a perpetual communication between the higher and lower regions of the atmosphere. The latter motion is incomparably more perfect in its effects than the former. The air is as cold at the height of three miles above the equator as at the pole, which is more than six thousand miles distant. And since the causes which produce a circulation in the fluid are concentrated within so narrow a compass, they must maintain an almost accurate equilibrium. It is immaterial what portion of the air be heated first, for the surplus heat will instantly be diffused through the whole of the vertical column. If a portion of air ascend, an equal portion must descend; so that the same *absolute* heat will be maintained at every altitude. Hence we may conclude, that the temperatures which prevail at different heights are reciprocally as the specific attractions for heat due to air of the corresponding densities. If  $d$  denote the rise of temperature which dilated air indicates upon recovering its density, and 2000 degrees be the distance of the thermometric zero, we may deduce the specific attraction of this rarefied air for heat to be  $\frac{2000}{2000 + d}$ , or  $\frac{2000 - d}{2000}$  very nearly when  $d$  is a small number. Consequently, if air of the usual density be allowed to expand itself into the state here supposed, its temperature would sink almost the same difference,

\* The distinction between summer and winter, in the high latitudes, was probably more strongly marked than even at present, by reason of the greater obliquity of the ecliptic.

*d.\** It appears, therefore, that the formula which we derived from experiments made on the collapse of air in the receiver of an air-pump, will apply likewise to the gradation of cold in ascending the atmosphere. The density at the surface being reckoned the unit, let its diminution at the given elevation be denoted by  $x$ , then the diminution of temperature on the ascent will be in degrees of Fahrenheit  $= \left( \frac{1 - \frac{1}{2}x}{1 - x} \right) 125 x$ , which may, perhaps, be expressed more conveniently thus,  $1000 x \left( \frac{4x - x^2}{32 - 32x} \right)$ .†

This formula corresponds, as well as might be expected, with the very few facts which I have been able to collect. Dr. Hunter reports, that at the elevation of 1400 yards on the Blue Mountains, in the island of Jamaica, the springs were  $18^\circ$  colder than on the level of the shore. The difference of density, at such an altitude and such a climate, may be computed to be  $\cdot 137$ , whence  $\left( \frac{4 \times \cdot 137 - \cdot 137^2}{32 - 32 \times \cdot 137} \right) 1000 = 19^\circ$ , which differs only one degree from observation. Lord Mulgrave mentions, in his voyage, that the air at the top of a mountain in Spitzbergen, 1503 feet high, was  $8^\circ$  colder than at the bottom. But the decrease of density may be estimated at  $\cdot 057$ ; whence  $1000 \left( \frac{4 \times \cdot 057 - \cdot 057^2}{32 - 32 \times \cdot 057} \right) = 7\cdot 5^\circ$ . I perceived the same agreement in some trials which I lately made on the springs in Fifeshire. I must confess, however, that though I am fully convinced that the formula expresses the true law of the diminution of temperature at different elevations, I still entertain a suspicion that the coefficient 1000 requires some correction. The glass receivers which I employed were not so much disproportioned as I should have desired; and the difference is so minute, on which the analogy turns, that I must have hesitated to publish the result without an appeal to observation.

Resuming the first formula  $\left( \frac{1 - \frac{1}{2}x}{1 - x} \right) 125 x$ , the part  $\frac{1 - \frac{1}{2}x}{1 - x}$  may be regarded equal to unit in small heights above the surface; and since the diminution of density is in this case nearly proportional to the ascent, the gradation of cold must,

\* If greater accuracy be required, let this equation be converted into an analogy,  $2000 + d : 2000 :: 2000 : 2000 - d$ , then  $2000 + d : d :: d : d - d$ ; whence  $d - d = \frac{d}{2000 + d} + 1$ . When  $d = 20^\circ$ , the correction will be  $= \frac{20^\circ}{51}$ ; and when  $d = 50^\circ$ ,

there should be an abatement of  $\frac{50^\circ}{41}$ , or somewhat more than a degree.

† The formula, which I have finally adopted from more perfect experiments, is  $45^\circ x \left( \frac{2 - x}{1 - x} \right)$ , in which the expression is a little modified, and the coefficient or multiplier diminished.



in our climate, amount to a degree, on an average, for every 220 feet.\* But this uniform progression will not extend to considerable elevations. It is hence easy to perceive that the rule for barometrical measurements, which directs the whole intervening column of air to be reckoned at the medium temperature of the two stations, must be inaccurate; and the more so, as the difference of a degree produces a variation of near  $\frac{1}{1000}$ th on the whole result. If it were not a digression from the main subject, I would proceed here to deduce the proper correction from our formula. The most exact method, however, would be to make observations with the barometer and thermometer at different stages, in ascending and descending the mountain. I cannot, on this occasion, help expressing my surprise that no attempts, as far as I recollect, have been made to estimate the effects of humidity, which must often be very great. I have lately inferred from theory, and verified it by experiment, that the air, in dissolving water, expands so much as to become specifically lighter. It would, therefore, be highly expedient to introduce the hygrometer in barometrical measurements. But no instrument of that kind, yet offered to the public, is constructed upon principles that are unexceptionable.† I shall probably resume the subject at another opportunity.

In applying the formula to ascertain the cold of elevated continents, some abatement should be made; for, in these cases, the communication between the higher and lower strata of the atmosphere, being necessarily distant and circuitous, must be imperfect. The climate of such territories will evidently partake of that which they would enjoy if depressed to the level of the sea. The heat derived immediately from the sun, which acts principally near the surface, being dispersed with unusual slowness, must continue to accumulate, till this very ascendancy produce an increased circulation of the atmosphere sufficient henceforth to maintain the equilibrium. Upon the same principles, the air upon the top of a mountain must be rather warmer than at the same height in the open plain. In like manner, a spot enclosed on all sides by lofty precipices, but yet accessible to the sun's rays, will become extremely hot: not because the light is reflected and concentrated by the rocks, as some have strangely supposed, but because the circulation of the air at the narrow basin is obstructed, and, consequently, the dispersion of the heat. Such are the delightful vales scattered among the Alps and Appennines. If these recesses of nature be encircled by naked rocks, the effects will be still more remarkable; for evaporation not only produces cold, but, by dilating the air, it promotes the diffusion of that influence.

\* This estimate is rather too large. It will be found nearer the truth, to reckon  $1^{\circ}$  of cold, on Fahrenheit's scale, for every hundred yards of ascent.—A.

† I could not then anticipate that I should have the good fortune to invent an hygrometer, of a construction at once simple and correct.—A.

As the great commerce of air between the poles and the equator, tempers, as much as possible, the inequalities of climates and seasons, so the circulation between the higher and lower strata preserves the balance of day and night. And how much more complete the operations of the latter motion than those of the former, may be judged from this circumstance,—that in our latitudes there is smaller difference of temperature between day and night than between summer and winter, though in the one case the sun is entirely withdrawn from us, though he sheds some light in the other; not to mention the vast disproportion of the times in which those different effects are produced.

When the sun shoots his rays vertically through a bright sky, a very large share of them will reach the surface of the earth, and there generate a heat which will greatly exceed the quantity diffused by the air. But, in the evening, the circulation which had been so violently excited by the meridian heats will restore the balance, and even produce an inclination to the opposite extreme. It is, hence, that the sultry days of the tropical countries are generally succeeded by evenings comparatively cool; a circumstance which proves so dangerous to new-settlers. Upon the whole, however, the difference between the heat generated at the surface, and that at a certain height in the air, is not so great as might be apprehended. If, in clear weather, the ground be much heated by a profusion of rays, it is likewise cooled by the evaporation, which is then most considerable. When an extensive tract affords not moisture, the accumulation of heat, on the surface, will be greater. Such are the sandy plains of Arabia Deserta. On the other hand, the condensation of vapours, which takes place in the lower regions of the atmosphere, must evolve much heat in the vicinity; and the clouds, absorbing almost the whole of the sun's light, become immediately warmed, and diffuse their influence all round. We are sensible of this fact \* \* \* \*

[Two or three concluding sentences of the Manuscript are unfortunately lost.]

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### ARTICLE III.

*Analysis of a Specimen of Water, taken out of a Boiling Spring, uniting with the Sea in the Harbour of Milto, one of the Grecian Islands in the Archipelago.* By T. Thomson, M.D. F.R.S.

THE bottle of water which I have subjected to analysis was put into my hands about two years ago, by the Rev. Mr. Holme,

28 Dr. Thomson's *Analysis of a Specimen of Water* [JULY, of Cambridge, who informed me that it was taken at the spot by Mr. Crystal. I was obliged to leave London shortly after I got the specimen, and was not again in possession of a laboratory till last winter, when I subjected it to the requisite trials to ascertain its composition.

### I. *Properties of the Water.*

The water was transparent, but there was a slight black flocky sediment at the bottom. It had the smell of sulphuretted hydrogen gas; but the smell was not much stronger than that of St. Bernard's Well, near Edinburgh. Its taste was strongly saline; but without the bitter impression yielded by sea water.

The specific gravity at 60° was 1.0331.

### II. *Action of Reagents.*

1. Muriate of barytes threw down a copious white precipitate which was not redissolved by nitric acid. Indicating the presence of *sulphuric acid*.

2. Nitrate of silver threw down a copious curdy white precipitate, which blackened on exposure to the light. Indicating *muriatic acid*.

3. Oxalate of ammonia threw down a copious white precipitate. Indicating *lime*.

4. A portion of the liquid freed from lime by means of oxalate of ammonia was mixed with some carbonate of ammonia, and then a drop of phosphoric acid was added; but no precipitate nor cloudiness was perceptible. Hence the water contained *no magnesia*.

5. A portion of the water concentrated by evaporation was acidulated with tartaric acid, and set aside for 24 hours; but no crystals of bitartrate of potash made their appearance. Hence the water contained *no potash*.

6. Neither pure potash nor quicklime when added to the water occasioned any evolution of ammoniacal fumes. Hence the water contained *no ammonia*.

6. Neither prussiate of potash nor sulpho-chyazate of potash occasioned any change of colour. Hence the water contained *no iron*.

8. The addition of sulphuric acid occasioned no effervescence. Hence the water contained *no carbonic acid* and *no carbonate*.

9. The water blackened silver, and threw down acetate of lead of a dark brown colour. It contained, therefore, *sulphuretted hydrogen*.

From these preliminary trials, I concluded that the saline constituents of the water were the following:

- |                           |          |
|---------------------------|----------|
| 1. Sulphuric acid.        | 4. Soda. |
| 2. Muriatic acid.         | 5. Lime. |
| 3. Sulphuretted hydrogen. |          |

III. *Analysis.*

1. Five hundred gr. of the water were evaporated to dryness, and the saline residuum exposed to a heat of about  $500^{\circ}$ . It weighed 22 $\frac{1}{2}$  gr.

2. From a bottle of water which had been taken out of the boiling spring, and which had remained in my possession for nearly two years, I had no reason to expect any great quantity of sulphuretted hydrogen gas. I put 12 cubic inches of it into a small retort, and boiled it for nearly an hour. The gas extricated did not amount to a quarter of a cubic inch; and of this only a small proportion was sulphuretted hydrogen. The tube containing this gas being wide, I did not attempt to determine the exact proportion of this gas that had been extricated; but was satisfied with ascertaining that the quantity was quite trifling.

3. From 500 gr. of the water, the sulphuric acid was precipitated by means of muriate of barytes. The sulphate of barytes after being washed and dried in a red heat weighed 1.12 gr. Indicating 0.38 gr. of sulphuric acid.

4. From 500 gr. of the water the lime was precipitated by means of oxalate of ammonia. The oxalate of lime obtained weighed 4.94 gr. It was heated to redness in a covered platinum crucible, mixed with sulphuric acid, and then exposed for some time to a strong red heat. The sulphate of lime thus obtained weighed 3.67 gr. Indicating 1.54 gr. of lime.

5. Five hundred gr. of the water which had been freed from their sulphuric acid and their lime, by the methods indicated above, were evaporated to dryness, and the remaining saline mass fused in a platinum crucible. It now weighed 21.58 gr. and was common salt.

IV. *Conclusions from the Analysis.*

I have no doubt, from the observations made by Dr. Murray, in his paper on the analysis of sea water, that all the sulphuric acid which I obtained existed in the water in combination with soda; and that all the lime was combined with muriatic acid.

Now an atom of sulphuric acid = 5, and an atom of soda = 4. Therefore we have  $5 : 4 :: 0.38 : 0.304$  = soda in combination with the sulphuric acid. Of course the sulphate of soda (supposed destitute of water) in 500 gr. of the water amounts to 0.684 gr.

An atom of lime weighs 3.625, and an atom of muriatic acid 4.625. Therefore, for the muriatic acid united to the lime, we have  $3.625 : 4.625 :: 1.54 : 1.965$  = muriatic acid in combination with the lime. Hence the muriate of lime in the water, supposing it anhydrous, amounts to 3.505 gr.

The whole of the common salt obtained by evaporating the 500 gr. of water, freed from their sulphuric acid and their lime,



did not, previous to the analysis, exist as such in the water; for the sulphate of soda was converted into muriate of soda, and existed as such in the 21.58 gr. of common salt. Now 0.304 gr. of soda require 0.352 gr. of muriatic acid to convert it into common salt; so that the common salt which must be subtracted from the residue amounts to 0.656 gr. Hence the common salt contained in 500 gr. of the water amounts to 20.924 gr.

Thus the anhydrous salts contained in 500 gr. of the water from the boiling spring are the following:

	Grains.
Common salt .....	20.924
Muriate of lime .....	3.505
Sulphate of soda. ....	0.684
	<hr/>
	25.113

Upon comparing the quantity obtained by the analysis with the weight of salts as deduced from the direct evaporation of a portion of the water, it will be seen that the analytical result exceeds the other by 2.453 gr. This difference is probably owing in part to the too rapid evaporation of the water in an open vessel, which doubtless occasions a loss; but I think it not improbable that a portion of the muriate of lime might have been decomposed or carried off by the sudden application of a strong heat to the salt. In the analytical processes, the heat was applied much more slowly, and, therefore, the results are more to be depended on.

The saline constituents of this water differ in several respects from those of sea water. They are more abundant, amounting to five per cent. whereas those in sea water do not exceed 4.5 per cent. This difference is indicated by the specific gravity of the water which is considerably greater than that of sea water. Another remarkable difference is the absence of the magnesian salts, which constitute so remarkable a constituent in sea water. The proportion of sulphate of soda is also considerably less, and that of muriate of lime considerably greater than in sea water.

These circumstances, together with the presence of sulphuretted hydrogen gas, can leave no doubt that the boiling spring, whose analysis I have given in this paper, is quite different from sea water.

I regret that I cannot lay before my readers any information respecting the nature of the island in which the spring occurs. Tournefort mentions boiling springs in the harbour of Milo, and describes the island as abounding in sulphur and iron mines; but I presume Milto is a different island from Milo, though Tournefort takes no notice of it.

## ARTICLE IV.

*On Native Hydrous Aluminate of Lead, or Plomb Gomme.*

By James Smithson, Esq.

Paris, May 22, 1819.

I SEE in the *Annals of Philosophy* for this month, which I have very lately received, an analysis by M. Berzelius of the mineral which was formerly known here under the name of "plomb gomme."

The first discovery of the composition of this singular substance belongs, however, to my illustrious and unfortunate friend, and indeed distant relative, the late Smithson Tennant. He ascertained when last at Paris, on pieces furnished him by M. Gillet de Laumont, that it was a combination of oxide of lead, alumina, and water.

At that time I received a small specimen of this rare ore from M. de Laumont, accompanied with a label, of which the following is a copy :

"Hydrate d'alumine et de plomb reconnu par Mr. Tennant, du Huelgoat, près Poullaouen, en Bretagne (Finistère) qui paroît être la même substance décrite par Rome de l'Isle, tom. iii. de la Cristallographie, p. 399, comme plomb rouge en stalactite.

"J'en ai dit quelques mots en Mai, 1786, dans le Journal de Physique, p. 385, F. 16."

This ore is of a yellow colour; it otherwise bears so great a resemblance to the siliceous substance found near Frankfort on the Mein, called Müllen glass, that it might be mistaken for it.

Suddenly heated, it decrepitated violently; but heated slowly, it became white and opaque. The utmost fire did not appear to fuse it, or produce any further alteration in it.

It dissolved readily in borax into a colourless transparent glass, but no reduction of lead took place. Not having any carbonate of soda at hand, I added a particle of nitre, whose deflagration producing potash, lead was revived.

A bit, which had been made white by ignition, being wetted with nitrate of cobalt and again ignited, became blue.

Heated in a glass tube over a candle, it decrepitated, became opaque and white, and water sublimed.

Mr. Tennant mentioned to me a sort of explosion occasioned by the sudden expulsion of the water, and characteristic of this ore, which took place when it was heated at the blow-pipe. With the very minute particles I have tried, no effect of this sort was perceived.

The above characters will prove sufficient, I apprehend, to make this substance known when met with.

## ARTICLE V.

*On Centrifugal Force, and the Upright Growth of Vegetables.*

By Mr. Henry Meickle.

(To Dr. Thomson.)

SIR,

London, May 26, 1819.

IN your number for April last, the Rev. Patrick Keith advances some objections to the hypothesis of T. A. Knight, Esq. to account for the direction of the radicle and germen. I do not pretend to decide in favour of either side of the question, but only beg to make some brief remarks on the effects of gravitation, and on Mr. Keith's singular ideas of a centrifugal force produced by rapid circular motion. Mr. Keith, in pages 253 and 254, seems to hold out that the centrifugal force acts in the direction of a tangent to the orbit, and not directly from the centre. Now this notion seems peculiarly his own, and admitting that it were correct, some curious consequences would necessarily follow. For instance, if it acted forward in the direction of the tangent (as is implied in his assertion) it must tend to accelerate the motion. But since centrifugal force is itself the result of motion, it and the motion would mutually tend to assist each other *sine limite*; so that a body once set a moving in a circle would not only persevere in its motion, but be accelerated *ad infinitum*; and, of course, neither so much time would have been wasted, nor so many brains cracked or rendered worse than love-sick with the delusive and idle search after the vain chimera of a perpetual motion.

To establish the well-known fact that the centrifugal force acts directly from the centre, nothing more is necessary than to attend to the simple experiment of whirling a sling around the head; or indeed any weight attached to the one end of a string, while the other is held in the hand, giving the weight a whirling motion, is sufficient to put it past the possibility of a doubt.

When a body is relieved from moving in a circle, its flying off in a tangent is in perfect harmony with, nay a consequence of, the centrifugal force's acting directly from the centre. But it would be presumptuous in me to enlarge on a subject so ably handled in the numerous works on the laws of motion and composition of forces. Had Sir I. Newton held any other opinion, he might have had the opportunity of seeing his immortal System of the Universe safely interred long before his death.

I do not see how gravitation could have had any effect on the vertical wheel at all; for although this power is regardless of motion, and acts without ceasing, yet its solicitations are not instantaneously obeyed, but require time to produce any sensible effect. No sooner, therefore, had gravitation desired a particle to move in any one direction than it instantly itself recalled the

mandate, by reason of the rapid revolution of the wheel. Gravitation, therefore, could have no sensible effect on the vertical wheel; but it would appear, that when the horizontal wheel made 80 revolutions per minute, the centrifugal force was just equal to gravity.

Indeed if the length of the radicle bore any considerable proportion to the radius of the horizontal wheel, it ought to have been a little curved, or more horizontal, toward the extremity; but the reverse would take place with the direction of the germen. Had great attention been paid to the direction of the growth, it would probably have been found a little pointed backward from the radius in both wheels, in consequence of the resistance of the air.

I have often observed in the growth of wood, that when a tree did not stand upright but leaned to a side, the annual growths or tubular coatings were much thicker on the under than upper side. In branches shooting out horizontally, the difference was still more remarkable; but this is sometimes counteracted by another circumstance—that when trees are very crooked, the hollow side of a turn fills up, or grows much more rapidly than the round side; so that the juices seem disposed to take the shortest road. This appears to be the reason why some sorts of trees, though extremely crooked when young, gradually straighten as they advance in years.

I am, Sir,

Your most obedient servant,

HENRY MEIKLE.

P. S.—There is still something inaccurate about the mean degree of latitude deduced from Col. Lambton's measurements, contained in your number for March. From the table there given, it appears to me that the degree at the latitude of  $45^{\circ}$  (usually taken as the mean) is  $69.0361$  miles, or  $69\frac{1}{8}$  nearly, which is still further from  $69\frac{1}{8}$ , than the former number. H. M.

\*.\* The length of a degree of latitude at  $45^{\circ}$  given by Col. Lambton is  $60751.8$  fathoms =  $69.036$  miles (the number  $69.030$  is by an error of the press for  $69.036$ ). When I said that the mean length of a degree of latitude was nearly  $69\frac{1}{10}$  miles, I meant for Great Britain, and particularly for London.—T.



## ARTICLE VI.

*Account of a newly discovered Variety of Green Fluor Spar, of very uncommon Beauty, and with remarkable Properties of Colour and Phosphorescence.* By Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, Member of the Royal Academy of Sciences at Berlin, &c. in a Letter to the Editor of the *Annals of Philosophy*.

(To Dr. Thomson.)

SIR,

I AVAIL myself of the first moment of leisure granted at the close of my annual course of lectures in this University, to fulfil my promise of sending to you an account of the *Durham Fluor*; of which I have received specimens surpassing in magnificence and in the beauty of their crystallizations, any mineral substance I have ever before seen. I am indebted to the *Rev. G. Peacock*, M.A. Fellow of *Trinity College, Cambridge*, not only for making me acquainted with all the circumstances of the discovery, but also for having had the goodness to procure for me the specimens to which I allude. The same gentleman, from notes written upon the spot, has also supplied me with materials for making the present communication. The name of the mine, whence this singular variety of the *fluat* of lime has been obtained, is *Middlehope Shields*; it is the property of *Colonel Beaumont*; and it is situate about  $1\frac{1}{2}$  miles to the north of the village of *West Gate*, in *Weardale*, in the county of *Durham*; about six miles to the west of the town of *Stanhope*. This mine has been worked with great success for a considerable time; but it was only during three months of the autumn of 1818 that this beautiful mineral was obtained in any considerable abundance. The excavations have since that time been partially interrupted, in consequence of the *vein* crossing another *vein*, which is the property of a company of miners, who are known in that county by the name of the *London Lead Company*. They are now employed in driving a level through the *intersecting vein*, and in sinking a shaft, for the more effectual ventilation of the mine.

The *rider* of the *vein* (a provincial term designating the substance wherewith the *vein* is principally filled) is a dark buff-coloured *magnesian limestone*; sometimes of a very friable texture; which is accompanied by masses of *fluor spar*; sometimes cubical, and sometimes amorphous; of different hues, *green*, *purple*, and *white*; all of which are not unfrequently exhibited in the same mass; the *white* occupying the centre, the *green* appearing towards the external surface, and the *purple* tinging the intermediate parts; but it is only in the *flats* (i. e. large openings or cavities) of what is called the *eleven fathom*,

or *great limestone*, that the finely crystallized and transparent *fluor-spar* is found. These cavities are invested with the substance of the *vein*, and the *fluor* is there found completely covering the sides; they are generally of inconsiderable extent, but sufficiently capacious to admit a man standing upright within the cavity. The *fluor* is there sometimes accompanied by cubical and dodecahedral crystals of the *sulphuret of lead* (*galena*), and sometimes by a little *quartz*; but, I believe, by no other substance.

The *vein-stuff* of the *intersecting vein* is a coarse kind of *purple fluor* and *calcareous spar*; but although the workings for this *vein* have been carried within 50 yards of the main deposits of the *green fluor* in the other mine, and in the same *limestone* (the richest of all the *metalliferous limestones* of this country) no similar substance has been observed. The only metallic substance ever found in these *veins* is the common *sulphuret of lead*, called *lead glance*, or *galena*; no *copper* has been discovered in the mines of *Weardale*.

Plate XCIV is a sketch of the situation of the *green fluor mine*, showing also the manner of its intersection by the *London Lead Company's Mine*.

Having thus stated the *geographical position* and *habitat* of this *fluor*, I will now proceed to detail its mineralogical characters.

The finer crystals are perfectly transparent. Their colour by transmitted light is an intense *emerald green*; but by reflected light, the colour is a deep *sapphire blue*; and this remarkable character causes such a play of the *green* and the *blue* light to a person regarding these crystals, that at first sight he is unable to say which causes the more beautiful appearance, and to which of these hues their real colour ought to be referred. Some of the crystals are of such magnitude that their *major diameter* measures two inches, but it rarely exceeds one inch. I have said *major diameter*, because many of them are *parallelopipedons*, with or without bevelled edges; their surface most beautifully exhibiting the lines of decrement, formed by the laminæ of superposition upon the primary nucleus. These *parallelopipedons* are universally accompanied with twin crystals, which sometimes exhibit a solid angle, salient, above the face of the original crystal; and sometimes one nearly as large as the crystal in which it is imbedded; the superior edge of the imbedded crystal being always inclined at the same angle (somewhat less than  $30^{\circ}$ ) to the face of the other; and its lines intersect the plane of the original crystal in lines which make when produced nearly equal angles with the edges or sides containing that angle towards which the prominent edge of the twin crystal declines. The twin crystal always displaces the parts of the edge which it intercepts, so that they are never in the same straight line, and rarely in the same plane; and this character in the crystallization

36 Dr. Clarke on a new Variety of Green Fluor Spar. [JULY, of this remarkable variety of the *fluat* of lime adds greatly to the peculiar play of light and beauty of the specimens.

Until these crystals were sent to Cambridge I had never seen crystallized *fluat* of lime in the form of *parallelopipeds*, neither does Mr. Jameson, in his valuable work on mineralogy, mention any such form ; \* their major diameter is to their minor diameter as three to two. The other forms exhibited by the same *fluor* is the *cubic*, with or without bevelled edges. The miners are assured of the neighbourhood of cavities lined with crystals by the increasing hardness of the substance of the vein.

This kind of *fluor* is also remarkably distinguished from every other variety by its *phosphorescence*; which is such that it becomes visible in a dark room simply by placing a large crystal in water heated nearly to the boiling temperature. But to exhibit its *phosphorescence* in perfection, it is necessary to reduce the mineral to powder in a mortar, and then scatter its particles upon the surface of an iron plate, heated nearly to redness. It then *phosphoresces* with a *violet-coloured* light. Mineralogists who have added to the nomenclature of the varieties of *fluor* by the introduction of the word *chlorophane* (as applied to the substance which emits a *green* light when heated), may, if they choose, call the *Durham fluor* by the name of *cyanophane*. Its other characters are those which are common to every variety of the crystallized *fluat* of lime. Its *specific gravity*, estimated in distilled water at a temperature of 65° of *Fahrenheit*, equals 3.14. Before the blow-pipe, when supported on *platinum* foil, it decrepitates, beautifully *phosphoresces*, loses its colour, becomes highly limpid, and is ultimately reduced to an opaque white enamel. Upon *charcoal*, its fusion is more readily accomplished; the results being the same. When first brought from the mine, it is extremely fragile; but (notwithstanding its natural brittleness and inferior hardness) when properly desiccated, owing to the intensity of its fine *green* colour, it is probable that lapidaries will cut and sell its transparent crystals as spurious imitations of *euclase* or *emerald*. I remain, Sir, faithfully yours,

Cambridge, May 10, 1819.

EDWARD DANIEL CLARKE.

P. S. The crystals sometimes form two *parallelopipeds*, having a common solid angle; in this case the twin crystal is found at the re-entrant angle, and its superior edge declines towards the common angle; although not always directly. Some rough crystals, which are not highly transparent, are marked with small cavities, in the form of the inferior pyramid of a regular *octahedron*.

\* See Jameson's Mineralogy, vol. ii. p. 222. Edinb. 1816. Among the remarkable instances of crystallization exhibited by the *fluat* of lime, there is one preserved in the Woodwardian Collection at Cambridge, exhibiting an *octahedron* formed by the juxta-position of cubes. I possess crystals of *Siberian fluor* with 26 sides.

## ARTICLE VII.

*Remarks on the Combination of Acids with Bases and indifferent Substances.* By Dr. F. Sertürner, of Einbeck.\*

IN both my dissertations on opium, morphia and meconic acid, there occur some conclusions respecting the compounds which the acids form with indifferent substances. To these I was led by observations not generally known, though I did not mention them. I shall here give a short account of these observations. In 1806 I found, during a series of experiments respecting the formation of sulphuric ether, and which appeared to me inexplicable, according to the usually received theory, that sulphuric acid, when simply mixed with alcohol, unites to it, and forms a peculiar acid which cannot be decomposed by any saline basis, to which I gave the name of *prot-oinothionic acid* (*first sulphuretted wine acid*). By considering this substance as an intimate compound of the acid with the alcohol, which cannot again be decomposed by heat, the nature of it becomes sufficiently evident. Now as nothing more is necessary but a small spark to set fire to a great quantity of combustible matter, this observation in like manner carried me speedily a great deal further. I easily conjectured and speedily ascertained that the powerful acids are capable of uniting not merely with alcohol, but with other organic bodies, as sugar, gum, tallow, when cautiously treated, and of forming with them acids more or less durable, and in many respects remarkable, though hitherto entirely overlooked. By these observations the field of chemistry was enlarged in a practical and theoretic point of view. It deserves attention, and is very characteristic, that no reagent indicates a trace of the substances of which these new acids are composed; that, for example, in oinothionic acid, and its salts, neither solutions of lead nor barytes indicate the presence of sulphuric acid by rendering the liquid muddy. Several chemists have in fact made experiments with these acids and their salts; but they have drawn wrong conclusions respecting them.

Prof. Trommsdorff has lately discovered and made known a true direct compound of alcohol with vegetable acids. This, as far as my knowledge reaches, is the first time that such a compound, which unites with the saline bases without undergoing decomposition, has been thought of. The strong mineral acids combine with it more easily, as they are capable of driving off the caloric; for alcohol is a thermate; or a compound of true alcohol and heat. With the hydrates the combinations take place more readily; because in them the water is not combined so firmly as the caloric is in the thermates. A circumstance,

\* Translated from Gilbert's *Annalen der Physik*, ix. 33. (Sept. 1818.)



however, occurs in this case, which I shall take another opportunity of explaining.

The light produced by these observations goes further, and at once withdraws the veil from many phenomena hitherto inexplicable; as, for example, the formation of ether and starch sugar; and shows us that they are the consequence of the mutual action of the substances on each other. It has been supposed that alcohol is capable of neutralizing acids just as the saline bases do; while chemists overlooked the true compounds of alcohol with acids, substituting for them the ethers; and pushing this view of the subject founded on false premises still further, they compared these combinations with those of the saline bases, and considered them as similar. It is an axiom founded on theory and observation, that an *indifferent* substance, as water, hydrogen, sulphur, alcohol, azote, &c. (and still less an acid substance) does not possess the characters of a saline basis, and is, therefore, incapable of completely depriving an acid of its acid properties.\* The substance produced by the union of such bodies, according to the laws by which elementary substances unite, should be always acid; and this accordingly is what happens; for the property of neutralizing belongs only to the saline bases, and indeed is confined to those which are soluble in water. By this, all our former notions respecting the properties of acids and bases are destroyed. These indeed were by no means well established, and would require elucidation.†

\* I class among *indifferent substances* all bodies which do not possess the property of neutralizing, which do not unite with the saline bases as acids, but form with the strong acids peculiar acids not commonly decomposed by the saline bases. The combination of alcohol with the strong acids gives us the best example of this kind of compound. The hydrous acids are analogous to the alcoholic acids; but are distinguished by this, that they combine only with the feeble saline bases without decomposition; but by the strong bases are decomposed, and the water disengaged. The acids act in the same way as the indifferent bodies to the strong acids. The double acids constitute an example.

† Instead of *affinity*, I employ the more accurate term, *attraction of the elements*, because all chemical phenomena are the consequence of the attraction of the elements; and the doctrine of chemical forces, I call *stochiology*. Stochiometry, as at present taught, points out stochiology, with which indeed it is identical. It constitutes the basis of chemistry, and embraces the whole science as a pure scientific system. It is founded upon the different properties of the atoms of bodies. Can there, for example, exist an intimate union between two bodies in which the activity comes all on one side, little or none at all being exerted by the other? On that supposition is one member of the compound more or less passive and free, and does it show this by exhibiting several of its properties, while the active constituent of the compound loses its properties in some measure, its force being employed in fettering the passive constituent? Thus oxygen predominates in the acids, but the radical predominates in the saline bases. Hence the violent action which takes place between acids and bases. The greatness of this disproportion, and the greatness of the attraction between oxygen and the radical, determine the strength of the acid and the saline basis.

Results show that this view of the difference in the attractions of bodies is founded in nature. In this way it is possible from the properties of bodies to draw conclusions respecting their constituents. Hence it becomes intelligible why the acid is deposited at the positive pole and the basis at the negative pole of the galvanic battery: why some substances, as arsenic and sulphur, form with oxygen

Chemists supported their opinions respecting the acids by the analogy of sulphuretted hydrogen, prussic acid, oxymuriatic acid, &c.; and with respect to the saline bases, on the contrary, by the *ethers* containing acids completely neutralized, the acid in them being disguised by the alcohol precisely as if it were a saline basis. But these ethers (thermates) contain only a portion of the alcohol, or at least the alcohol in an altered state. Besides, it may be demonstrated that the two constituents cannot be separated as in the salts by a strong saline basis; nor when separated can they be again united into ether. These facts show us that the case must be different from what is commonly supposed. The following general observations will probably throw some light on the subject.

Alcohol and almost all the substances belonging to the class of indifferent or acid bodies, whether derived from the organized or unorganized kingdoms of nature, are incapable of saturating acids, or of destroying the acid property of bodies; but these bodies, in most cases, unite themselves to the strong acids in the same way as the saline bases do, without losing any of their constituents, except heat and water. The compounds produced by this union still continue acid, and are generally distinguished from the salts by this circumstance, that they cannot be decomposed by the action of the most powerful saline basis. By such combinations *peculiar acids* are produced, which have not the smallest resemblance to the acid, which enters into them as a constituent, and to which they are inferior in strength. To this class of bodies belong the alcoholic acids and all the acids produced by the combination of a strong acid with an indifferent substance.\*

The *subhydrates* and *hydrates*, or the combinations of the acids with the first doses of water (an indifferent substance), which are necessary to them as a basis, have the greatest resemblance to the alcoholic acids, and belong to the same class with them; only in these hydrates the water (the second constituent of such

acids; while others, as lead and calcium, form with it saline bases; and why acids and saline bases endeavour so eagerly to unite together. Hence likewise we can form a general notion of what an acid and a saline basis is. Hydrogen, sulphur, and similar bodies, are in the same situation. That sulphur forms an acid with hydrogen ceases to be surprising when we know that sulphur itself possesses the properties of an acid, and that it is capable of neutralizing acids. It would exhibit the properties of other acids, and be still a stronger acid than sulphuretted hydrogen, if it were soluble in water. Hydrogen communicates to it the property of dissolving in water, and of course renders it capable of reddening litmus. It is already established that a body destitute of the properties of a saline basis is not capable of neutralizing acids. The consequence of this must be, that the acid sulphur, when united with the indifferent body hydrogen, must still constitute an acid, and that when united with the saline bases, it must constitute a salt. Sulphuretted hydrogen of course may be compared with the alcoholic acids. It is the sulphur which gives to these last acid properties. Cyanogen, whose acid nature is weakened and neutralized in the thermate by heat, is in a similar situation.

\* The alcoholic acids overturn the opinion that alcohol possesses the property of neutralizing acids like the saline bases. Even those who will not admit that heat is a constituent of ether, must at least allow that the ether formed by the action of an acid does not contain alcohol, but only the elements of that substance.

acids) is not so intimately combined as the alcohol in the alcoholic acids. We must, therefore, give them the name of *hydrous acids*, in the same way as we call the others alcoholic acids; for they combine without any decomposition with the weak saline bases forming hydrous salts. With the strong saline bases, they do not form such compounds; for when they unite with them, the water, their second constituent, is separated and driven off. Thus we are acquainted with hydrous sulphate of alumina, but with no real anhydrous sulphate of alumina; for alumina is not capable of separating sulphuric acid from water. We know likewise, from the doctrine of definite proportions, that alumina ought to combine with a greater proportion of sulphuric acid than it actually does; for water not merely diminishes the strength of acids, but likewise their capacity for the saline bases; as is the case with the alcohol in the alcoholic acids. The truth of this assertion is proved by the neutral protoinethionate of lime, which, when heated, is converted into neutral sulphate of lime, while a portion of free sulphuric acid is disengaged.

As the water is only weakly combined in the strong acids, the powerful saline bases, as barytes, potash, &c. separate it, and form, when placed in contact with hydrous sulphuric acid, pure sulphates. Hydrous sulphate of the powerful saline bases do not exist. The hydrates of these are nothing else than combinations of the salts and water. In the alcoholic acids, the alcohol is more firmly combined than the water in the hydrous acids; accordingly they form alcoholic salts both with the strong and weak bases, without any exception; and we have not merely alcoholic sulphate of alumina, but likewise alcoholic sulphates of barytes and lime.

These observations throw light upon the dispute which exists at present respecting the compounds of chlorine and hydrochloric acid (as it has been called). The weak saline bases, which are not capable of separating the water from the muriatic acid, unite with it so as to form hydrousmuriates; but the strong bases form real muriates, possessing the property of separating the muriatic acid from the water. Hence it happens that while the salts which contain the hydrous acid are easily decomposed by heat, the others, on the contrary, are not in the least altered by heat; for as the hydrous acid contains in the water a basis, according to the condition of its constitution, it does not stand in need of the saline basis, being already united to the water. The heat does no more than separate the saline basis, and unite itself to the hydrous acid in its stead, constituting a thermate in which the heat is united to the hydrous acid like a base. The hydrous and alcoholic acids are always weaker than the strong acids of which they are composed; while the water and the alcohol diminish their capacity for the saline bases.\*

\* The volatile acids, which contain *heat*, and being united with it into thermates, do not require water, are not to be ranked with these. Heat in them con-



The strong acids form, as has been shown, with alcohol the first alcoholic acids, which are less powerful. Most of these, when exposed to the action of heat, are decomposed into two new compounds; that containing the caloric, *the ether thermate*, for example (sulphuric ether) comes over, and that containing the water, *the hydrous acid*, for example (the second oinothionic acid) remains behind. This compound of water is often, by exposure to the air, converted by absorption of oxygen into a third acid (third oinothionic acid), and sulphuric acid. The last two alcoholic acids contain only fragments of the alcohol; but they contain all the elements of sulphuric acid. The ether, on the other hand, contains none of the constituents of the acid. Hence we must conclude that it is a compound of caloric and of the fragments of the alcohol. But to this there are many exceptions.

The salts which these new acids form with the saline bases are almost all very soluble, and become still more interesting on account of the products which they yield when exposed to the action of heat. These very remarkable acids and their salts are easily obtained by mixing cautiously the strong and concentrated acids in excess with organic bodies, and by saturating the diluted liquids with carbonate of lime, of barytes, or any other base, and then evaporating them in a very gentle heat with an excess of the base. These acids, when subjected to distillation, yield acid and ethereal products. Their salts yield likewise other remarkable acids, which have some resemblance to meconic acid, succinic acid, and rhussic acid.\*

The neutral ethers, which contain an acid, cannot be compared with the binary compounds of alcohol and an acid, which always possess acid properties. They are triple compounds, into which an imponderable body, heat, enters as a true constituent, after the manner of the ponderable bodies, and with the assistance of the altered alcohol holds the acid so closely fettered, that the saline bases are not capable of disengaging it. Hence the separation of the constituents of ether is a difficult process. Thus, for example, the compound of the most concentrated alcohol and muriatic acid is an acid, and analogous to oinothionic acid, being, like it, weaker than muriatic acid. Muriatic ether is quite a different thing; namely, a product, formed by an alcoholic muriate destroyed by heat; and consequently, like sulphuric ether, is a compound into which heat enters as a constituent, and plays an important part, since, in company with the altered elements of the alcohol, it neutralizes the acid of the ether, and keeps it so firmly fettered, that the

stitutes a sufficient basis. The gas, or the vapour of these acids, on the contrary, are always thermo-hydrates. The water appears to constitute in them a first basis.

\* This acid, similar to succinic acid, is found abundantly in the berries of the *Rhus typhinum*, which ripen in the winter. It has in some cases a stronger affinity for lead than sulphuric acid has. I shall give a more particular account of it hereafter.



strongest bases are not capable of disengaging it. I name these compounds *thermates* and *subthermates* (from the analogy of *hydrates* and *subhydrates*), because in them the heat is as notable an ingredient as water is in the hydrates.

Many compounds to which the name of *hydrate* is given do not correspond with these characters; for a true hydrate is a compound of a substance with *thermate of ice* (water). The heat in these compounds is a more important and more active ingredient than the ice. Such are the true hydrates. From them we must distinguish the compounds which do not contain heat as a constituent, but simply ice; as, for example, *the crystals of salts*. This circumstance explains clearly and fully why different bodies when they unite with water evolve a high degree of cold or of heat. Suppose a substance which has a strong attraction for water, hydrate of ice (as crystallized muriate of lime), to be placed in contact with ice, the salt, in consequence of its attraction for water, obliges the heat (to speak figuratively), which is in the neighbourhood, to enter into combination with the ice, and form a hydrate of ice, in order to satisfy this attraction. The salt thus saturated with thermate of ice constitutes a true hydrate. Consequently the cold produced must be the more violent the greater the attraction of the salt for the thermate and the less heat there is in the neighbourhood; that is to say, the colder the mixture was. If, on the other hand, a substance, as lime, which has a stronger attraction for ice, be placed in contact with the thermate of ice, it separates the heat from the thermate, and unites itself with the ice in its place. Hence the solid hydrate of lime is not a true hydrate, but a compound of lime and ice. This is the reason why heat is evolved when such a compound is formed. The compounds which the oxide of hydrogen forms in the state of ice must be carefully distinguished from the hydrates or the compounds into which water enters in the state of water. We shall see hereafter that there are other subdivisions of these last.

I come now again to the *thermates*. They may be very well compared with the salts. Thus in neutral compounds of heat with free anhydrous acids, as thermate of carbonic acid, of sulphurous acid, &c. the heat is the only basis. There exist also compounds of heat and hydrous acids, which may be more or less saturated with water; and in them the water commonly performs the part of the principal bases. The vapour of the sulphuric acid, which does not smoke, and which of course is fully saturated with water, is the thermate of hydrous sulphuric acid. On the other hand, the gas or vapour of the dry sulphuric acid is a compound of heat with sulphuric acid not fully saturated with water. In this dry, volatile, sulphuric acid, crystallized like amianthus, the heat and the water appear to constitute equally the basis. Hence it may be considered as a *subhydrothermate* of sulphuric acid, or as sulphuric acid united at once

with heat and with water. There exist likewise other solid compounds, which contain heat, and, as it seems, light likewise (for where heat exists, light may unite also). To these belong sulphur, &c.

How necessary heat is to many bodies to render them permanent appears from carbonic acid (thermate of carbonic acid), which is a simple compound of heat, or another basis, and would probably be decomposed, as alcohol and several similar thermates are, if it were possible to deprive it of the heat which gives it the gaseous state, without, at the same time, uniting it with another equivalent basis; for from the relation of carbon to oxygen and to its compounds; namely, oxygen gas (thermate of lightoxide) and oxymuriatic acid, it is evident that only a weak attraction exists between them, which is overcome by light, since carbon is only able to separate the light-or-fireoxide (the basis of oxygen) by the assistance of heat; for heat is favourable to combustion, and occasions it in consequence of the attraction between carbon and lightoxide. The union of carbon with oxygen, therefore, can only take place in consequence of the influence of a third substance; and for that reason carbonic acid is always a compound of heat or another basis, which of itself has too little affinity for oxygen to decompose oxygen gas.\*

As carbonic acid and almost all the gases are to be considered as compounds of heat, there exist in like manner many other bodies, which are composed of three or four constituents, in which all the ingredients urge to union in order to satisfy their attraction for each other. Thus all oxides (as of

\* From this proceeds in part the enigmatical relation of oxymuriatic acid to carbon, notwithstanding the great mass of heat which it contains. *Oxymuriatic acid* is a double compound of light-or-fireoxide and muriatic acid, just as muriatic acid is a compound of muriatic acid and water. Now as carbon and oxygen unite only by means of heat or some other basis, still less is carbon capable of decomposing the triple compound in which oxygen exists in oxymuriatic acid (namely, *light*, or the basis of lightoxide, lightoxide in muriatic acid; and this double compound again united with heat), even when assisted by heat; because the more heat is thrown into this acid, the firmer do its constituents remain united; for oxymuriatic acid in its state of thermate is saturated with heat after the manner of a base. As in the preparation of oxymuriatic acid, the oxygen of the nitric acid or oxide of manganese separates the water from the hydrate of muriatic acid, and unites with the acid into a double compound, which we call oxymuriatic acid; it follows that muriatic acid has a stronger affinity for oxygen than for water. Carbon and heat, therefore, would be able to decompose the hydrate of muriatic acid more easily than oxymuriatic acid, even if this last had not so strong an attraction to heat as it has. The combination would likewise take place, and carbon would separate the light from the lightoxide, if muriatic acid had a strong affinity for carbonic oxide, and if the carbon, in consequence of its affinity for heat, were capable of assuming the gaseous state. We can draw no conclusion from the thermate of carbonic oxide, because in it the oxygen seems to serve as a medium of union between the carbon and the chlorine. The reason why carbon, the metals, hydrogen, sulphur, and all the other combustibles, have such different relations to oxygen, and when united with that principle in the same volume and the same proportion, form sometimes acids, sometimes bases, and sometimes indifferent substances. I propose to show in another place, in an article on the attractions of the elementary bodies.

platinum and gold) whose bases are not in a state to be separated without the assistance of another body, perhaps of a compound of oxygen, as oxygen gas, water, &c. are compounds containing a third body, which owes its presence to this.

I now return again to the combinations of acids with acids and indifferent substances. What is already known respecting these combinations, we owe chiefly to the valuable labours of Chevreul, Thenard, and some other modern chemists; but the facts which they have stated do not consist in direct combinations of these bodies, but of what may be called combinations of the fragments of their constituents. The same observation applies to the valuable researches of Bauhoff on the relation of alcohol to oxalic acid. They have not to do with alcoholic acids, the compounds which I propose to describe; but with compounds of the fragments of organic bodies with acids, which are formed only by difficult processes.

*Appendix concerning Alcoholic Acids, and Acid Compounds derived from the Union of strong Acids with indifferent Substances.*

The preceding essay gives the method, very briefly indeed, but clearly, of forming the new acids with a double base; but I consider it as still necessary to add some observations on the method of preparing these acids, and of the precautions which are requisite in order to ensure success, though the manner of forming them all is in some respects the same, yet every one requires some steps which are peculiar to itself. When muriatic acid is employed, the water requires to be removed by means of sulphuric acid, or, still better, by means of nitric acid and chlorine.

*Protoinothionic Acid.*

Two volumes of absolute or very strong alcohol are to be mixed with one volume of concentrated smoking sulphuric acid. The alcohol and sulphuric acid let water and heat escape at the same time, and unite together, constituting protoinothionic acid. It is now to be heated, and to be slowly saturated with chalk, after having been previously diluted. A little gypsum is formed which is separated from the liquid by filtration through fine linen and washing with water. The liquid is now gently evaporated with a small addition of chalk; and as soon as a crust begins to appear on the surface, it is filtered while still hot. On cooling, *protoinothionate of lime* separates in soft plates. This salt possesses a peculiar acid taste, attracts moisture from the atmosphere, takes fire when held in the flame of a candle, but is a permanent salt at the ordinary temperature of the air. When strongly heated, it becomes black, and exceedingly sour. This circumstance deserves attention, because it seems to be the chief reason why this acid, upon which so many persons have been employed in making experiments, was not sooner discovered.

This decomposition, indicated by the evolution of the black colour, commences by a gentle evaporation in the upper part of the vessel, as soon as some salt has made its appearance in that place. If, in the first part of the process, an addition of chalk be not employed, a complete decomposition takes place, and the liquid, which did not at first indicate the presence of a trace of oinothionic acid, now contains a great excess of it. This protoinothionic acid, which is easily separated from its combination with lime or barytes by means of sulphuric acid, contains within itself the whole mystery of the formation of ether; for it gives out *sulphuric ether*. Many chemists maintain that sulphuric acid by its action on alcohol loses nothing of its capacity of saturation. They draw their conclusions probably from the acidity of the mixture before and after the distillation; but the result would turn out otherwise if the acid were examined before it has united with the alcohol.

Almost all organic bodies and the strong acids exhibit the very same phenomena as we have just described. Accordingly as we employ subhydrated or hydrated acids, we obtain either perfect or imperfect acids. Whether the alcoholic acids be in this predicament, I do not know. From the oinothionic acids, I presume the contrary.

The great extent of the set of bodies on which my observations have been made has hitherto prevented me from entering into minute details, and has confined me to trials upon considerable masses of matter. Some interesting observations might have been made on this subject. It would have been curious to know how dry sulphuric acid would act when mixed with absolute alcohol, and whether, as is highly probable, together with protoinothionic acid, there be formed at the same time a quantity of common hydrous sulphuric acid. I suppose that the water separated from one portion of the acid by the alcohol unites itself to another portion of the acid, and forms the common hydrate of sulphuric acid. This becomes still more probable when we consider that a mixture of very concentrated sulphuric acid and absolute alcohol in which there was a great excess of the latter still acted as sulphuric acid.

#### *Deutoinothionic Acid.*

The residue, after the distillation of ether, which has been several times treated with alcohol and completely freed from ether, but still fresh, and not black or decomposed, is to be saturated with chalk, and treated as before directed. The deutoinothionate of lime thus obtained has a very sweet taste, and possesses the peculiar property, when exposed to the air, of absorbing oxygen and being decomposed into a great deal of concentrated sulphuric acid and tritoinothionic acid. Hence we see the reason why the residue from the preparation of ether,





when exposed to the air, recovers again the property of giving out ether which it had lost, and why it is then capable of saturating a greater quantity of a saline basis than it was before. The deutointhionic acid is decomposed as well as its salts into much sulphuric acid and the tritointhionic acid. It is formed indirectly by the action of heat from the protointhionic acid, a portion of the elements of alcohol being removed by the superior power of the heat. Hence arises a disproportion between the too great quantity of sulphuric acid and the too small quantity of the elements of alcohol, which occasions the decomposition of this acid in the air.

#### *Tritointhionic Acid.*

This acid is most easily procured by exposing the second acid, or the exhausted residuum after the distillation of sulphuric ether, to the air till it ceases to absorb oxygen. The diluted liquid is then saturated with chalk and evaporated. The tritointhionate of lime deliquesces in the air, but is a permanent salt, and, like all the alcoholic salts, is combustile. Its taste is nearly the same with that of the protointhionate of lime, to which salt indeed it bears a strong resemblance.

The alcoholic acids are in this way easily separated from the lime or barytes with which they are combined. When distilled, they yield other products. The three oinothionates of lime yield, when distilled, three new volatile crystallizable acids, similar to succinic acid and to rhussic acid, sulphurous acid gas, liquid sulphuric acid, and an exceedingly pleasant smelling ethereal gas. The remaining acids formed by the union of the strong acids with organized bodies give similar products.\*

I have thus exhibited to the eyes of inquirers a land not hitherto traversed, which promises a rich harvest of discoveries to those who choose to enter upon it. The statements contained in this paper are sufficient, I conceive, to justify me in this confidence. Meanwhile the new acid compounds thus detected do not constitute the whole of the novelties to be expected from this kind of research. Kirchhoff's starch sugar from amylo-thionic acid shows us that other new bodies are likely to be discovered by further researches.

\* That the acids and the salts containing nitric acid, united to organic bodies, give out likewise substances containing azote, I conjectured, when I distilled protiononitrate of lime; for the vapour which came over occasioned in me a very severe malady. This was the case likewise with meconic acid. But more of this in another place.

## ARTICLE VIII.

## ANALYSES OF BOOKS.

*Recherches sur l'Identité des Forces Chimiques et Electriques.*  
 Par M. H. C. Ørsted, Professeur à l'Université Royale de Copenhague, et Membre de la Société Royale des Sciences de la même Ville, &c. Traduit de l'Allemand par M. Marcel de Serres, Ex-Inspecteur des Arts et Manufactures, et Professeur de la Faculté des Sciences à l'Université Imperiale; de la Société Philomatique de Paris, &c. Paris, 1813.

(Concluded from vol. xiii. p. 463.)

## CHAP. VI.—Of the Production of Light.

Our author considers it as established that both light and heat are produced by the same kind of forces; but in a different state of activity. The evidences are, that it is a general law that every body, when heated to a certain temperature, becomes luminous; and that, on the other hand, when light falls on a body without being reflected, the body becomes hot. To elucidate this view of the subject, he goes over the different phenomena of light, and shows that they all agree with his theory, and serve to confirm it. With respect to the colours emitted by a burning body, he is of opinion that when the force of combustibility preponderates, the light emitted is blue or violet; when the burning force preponderates, the colour is red or yellow; and when they are in equilibrio, the light is green if the combustion be feeble, and white if it be strong.

I may remark that some of the consequences naturally flowing from this theory are hardly reconcileable with the present state of our knowledge. Heat and light ought not to pass through a perfect vacuum; yet we know that electricity at least appears in the torricellian vacuum. This last fact seems scarcely consistent with the notion that the electric spark is merely incandescent air.

## CHAP. VII.—Observations on the Existence of the General Forces in Organic Bodies.

The phenomena of animals (for those of plants have not been examined under this point of view) exhibit abundance of proofs of the existence of the general chemical forces in them. The nerves are excellent conductors of electricity in the living animal, and in conjunction with the muscles are capable of forming a kind of galvanic circuit. Prof. Ørsted conceives that the galvanic pile may serve to give us a kind of imperfect idea of the way in which organic bodies act. He thinks that muscular motion may be accounted for by the galvanic energy exerted by the nerves on the muscles.

CHAP. VIII.—*Of Magnetism.*

Magnetism has been always considered as analogous to electricity. Its phenomena of course must be the result of the action of the same kind of forces. There is only one difficulty that occurs to oppose our concluding that the electric and magnetic forces are the same, and that is, that electric bodies act upon magnetic bodies as if they were not endowed with any peculiar force. Our author not being able to deny the existence of this difficulty, endeavours to elude by pointing out some phenomena in electricity itself of somewhat an analogous nature.

He notices in this chapter an opinion of Ritter, which has since been confirmed; namely, that the aurora borealis is a magnetical phenomenon. Hansten, a Danish mathematician, has proved that the earth has four magnetic poles, as Halley had conjectured. He has found that the aurora borealis appear always in their greatest intensity round these four poles, and that the radii are parallel to the magnetical inclination.

CHAP. IX.—*Considerations on the Theories in the Experimental Sciences, and Synoptical View of the Principles of the Dynamic System.*

After some very judicious observations on the way in which the experimental sciences advance, and on the necessity under which we are to connect the facts in them by means of theory, our author terminates his work by the following epitome of the theory which he has endeavoured to establish in it.

Two opposite forces exist in all bodies, and of which they never can be altogether deprived. Each of these forces has an expansive and repulsive action in the medium in which it predominates; but they attract each other, and produce a contraction when they act on each other. The most free action of these forces produces the electric phenomena. These forces may be condensed, retained in a certain space, and even rendered entirely latent, by their attraction for each other.

Their passage from one point to another is so much the more difficult the greater their quantity and the smaller their intensity is. But when they are at once feeble, in great quantity and latent, or almost latent, they cease to be transmitted even by contact, if this change is not favoured either by the intervention of very intense forces, or by an augmentation of the conducting power.

It is in this state when the forces are too latent to produce the electric phenomena that they constitute the chemical properties of bodies. The way in which these two forces are disposed, and the state of cohesion and of conductivity which proceeds from it, as well as degree of preponderance of one of these forces over the other, form the principal differences which exist between bodies. The quantity of the preponderating force is always very

small in comparison of those which are in equilibrium in the body.

The forces which act as chemical properties, or, in other terms, as affinities, exist in bodies in three principal forms, which give us three series of affinities; viz. of bodies not burnt, of burnt bodies, and of salts.

In each of the first two series, there are antagonist bodies distinguished by the excess of the one or the other of these two forces. The combination between two similar bodies does not make them leave their series of affinity, while the union of two antagonist bodies makes them pass into the following series.

The same force which produces combustibility in the first series, produces alkalinity in the second; and the force which in the first series is burning, gives in the second acidity. Thus every body which has undergone combustion contains at once alkalinity and acidity, one of which is often too weak to be perceived.

The intensity of the alkaline and acid forces depends upon the excess of the predominating force, and on the liberty which it enjoys; their quantity, or which comes to the same thing, their capacity for the antagonist body, depends, on the contrary, in the alkalies, on the quantity of burning force, and appears to depend in the acids on the quantity of the force of combustibility.

The greatness of a chemical action ought to be in a ratio composed of the intensity and the quantity of the forces, abstracting, however, the circumstances favourable or unfavourable to the combination, as the heat, the state of the combination, &c.

Whenever the two forces unite in circumstances in which they cannot be perfectly conducted, heat is produced; whether we unite the electric forces disengaged by our apparatus, or make them unite the moment they are disengaged, as in friction, or put them in action by chemical combinations.

When the conducting power of a body diminishes, there is likewise a disengagement of heat.

When, on the contrary, the conducting power increases, the consequence is a diminution of temperature.

The propagation of electricity being produced by disturbing the equilibrium of the natural forces in the bodies, and this disturbance being the greater the more violent the transmission is; heat, which is merely an effect of this transmission, ought to consist in a disturbance of the equilibrium.

The equilibrium of forces is always accompanied by a contraction; therefore, the disturbance of this equilibrium produced by heat ought to diminish the contraction; that is, dilate bodies.

Hardness, brittleness, solidity, having for their cause a certain direction of the interior forces, or a determined situation of the molecules of bodies, these properties ought to be weakened, and



at last destroyed, by the disturbance of equilibrium produced by heat.

It is by the same disturbance of equilibrium that bodies in general become more decomposable at a high temperature, more proper to enter into combination with others, and at the same time better conductors of the electric forces.

The calorific action may pass into the state of light; and, *vice versa*, the luminous action may pass into that of heat.

The calorific action approaches more and more to the state of light in proportion as the reaction of the forces in it becomes more intense; which supposes likewise a greater velocity. The velocity of calorific, luminous, and chemical rays, follows according to our principles, as well as those of optics, the order of their refrangibilities.

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## ARTICLE IX.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

**April 22.**—A paper, by Capt. James Anderson, R.N. was read, entitled "Some Observations on the Peculiarity of the Tides between Fairleigh and the North Foreland, with an Explanation of the supposed Meeting of the Tides near Dungeness." After some general remarks on the common opinion respecting the meeting of the tides between Dungeness Point and Rye Harbour, Capt. A. proceeded to describe the peculiarity of the channel at that point, and its very sudden contraction between Dungeness and Cape D'Alprée, and between the South Foreland and Calais Point. In consequence of this contraction, the western tide, according to the author, meets with a resistance in its course at Dungeness and Cape D'Alprée; and the water must continue to accumulate until it deepens and widens the channel so as to become adequate to its discharge. To this accumulation, the author principally referred the peculiarities of the rise and fall of the tides in the neighbourhood of the above-mentioned places. That the tides do not meet at Dungeness in a line across the channel is further proved by the absence of that violent concussion of the waters which would occur in such a case. The fact is, according to Capt. A. that the formation of the coast by gradually altering the course of the tides between the South Foreland and the Buoy of the Nore from E.N.E. to W.N.W. occasions a gentle blending of the waters, so that there is a strong eddy about the Kentish Knock, and a foaming rippling where they meet and proceed together.

At this meeting, a paper was also read, by Sir E. Home, on the

ova of the opossum tribe. After alluding to the formation of the ova of quadrupeds, as described by him on a former occasion, the author proceeded to the subject of his present paper. The ova of the opossum tribe are formed differently from the ova of quadrupeds, and constitute two distinct gradations between that class of animals and the *ornithorhynchus paradoxus*, which last approaches so near the bird as to complete the series between quadrupeds and birds. The formation of an ovum in the kangaroo was first described. This, when expelled from the corpus luteum, receives a yolk in the fallopian tube, and subsequently an albumen in the uterus. The foetus, when removed from the uterus into the marsupium, attaches itself to the nipple as formerly described.\* In the wombat, and the great and small opossum, instead of corpora lutea, yolk bags are imbedded in the substance of the ovarium, and there are two uteri with a fallopian tube to each, the ovum in each uterus being separately impregnated in its own cavity. The mode of formation of the ova in the *ornithorhynchus paradoxus* constitutes the intermediate link between the opossum and bird. In this animal, the yolk bags are imbedded in the ovaria, and instead of a regular uterus, each fallopian tube swells out into a cavity in which the ova are impregnated.

A paper was read, by J. F. Wood, Esq. entitled "A Case of a Blue Child, with the Dissection." This child lived 21 months. On removing the pericardium after death, a large vein was observed descending on the left side of the thorax, and terminating in the right auricle of the heart, in which the superior vena cava also terminated by a distinct opening. The auricle was large, and the foramen ovale pervious. The aorta and pulmonary artery arose from the right ventricle, the cavity of which was likewise large and strong, and had no communication with the left, except by a foramen through the septum, which divides the ventricles. Such were the chief peculiarities observed in this case, which were illustrated by drawings.

There was likewise read at this meeting, a paper, by W. Morgan, Esq. entitled "Observations on the new System of diagonal Framing, introduced into his Majesty's Navy by R. Seppings, Esq." After some general observations upon other suggested improvements, the author proceeded to state his reasons for preferring Mr. Seppings's principles, and to point out the advantages arising from their adoption. The paper was accompanied by drawings illustrative of the subject.

*April 29.*—A paper, by Dr. Brewster, was commenced, on the optical and physical properties of tabasheer.

*May 6.*—Dr. Brewster's paper was concluded. After mentioning the mode of formation and chemical properties of tabasheer, Dr. B. proceeded to state that there are three varieties of this substance; one possesses a milky transparency, and reflects

\* See Phil. Trans. vol. lxxxv. and c.

a bluish light; another is transparent; and the third opaque. The first two varieties give out air, and become transparent when placed in water. The third likewise gives out air, but remains opaque. If the first varieties be only slightly wetted, they become quite opaque. The first two varieties resemble the hydrophanous opal in the property of becoming transparent when put into water, but differ from it remarkably in becoming opaque from the application of a small quantity of that fluid. As tabasheer disengages more air than the opal, its pores must be more numerous; and, therefore, the transmission of light, so as to form a perfect image, indicated either a very feeble refractive power, or some peculiarity in the construction of its pores. Dr. B. accordingly found by experiment that this curious substance has a lower refractive power than any other solid, or even liquid, and that it holds an intermediate place between water and the gases. Numerous experiments were detailed with the view of determining the power of tabasheer to absorb different fluids, and to ascertain the corresponding effects produced upon its optical properties, specific gravity, &c. The paper was concluded with an attempt to explain the cause of the paradox exhibited by the first two varieties of tabasheer, of becoming opaque by a small quantity of water.

*May 13.*—A paper, by T. A. Knight, Esq. was read, upon the different qualities of spring and winter felled oak trees.

The author concluded from his experiments and observations that in all cases where it is essential to give durability to the alburnum of the oak, the tree should be barked in the spring, and felled the ensuing winter.

At this meeting also, a paper, by Dr. Marcet, was begun, entitled "On the Specific Gravity and Temperature of Sea Waters in different Parts of the Ocean and in particular Seas; with some Account of their saline Contents."

*May 20.*—Dr. Marcet's paper was concluded. The object of this interesting memoir was to determine the general properties of sea waters in different parts of the ocean and at different depths, with the view of ascertaining whether they differed from one another, and in what respect. The investigation was begun several years ago in conjunction with the late Mr. Tennant, and in order to render it as complete as possible, every opportunity was taken to collect specimens from all parts of the globe. After these preliminary remarks, Dr. M. proceeded to describe the instruments which had been contrived to raise water from different depths, or from the bottom of the seas when practicable, and which had been employed in collecting the different specimens. The three great points kept in view in the course of the examination were, their specific gravity, their chemical composition, and their temperature, when this could be ascertained; and the author gave a circumstantial detail of the precautions attended to in the investigation of each of these important points. The results of the author's numerous observations and



experiments were arranged in the form of tables, and consequently did not admit of being detailed. The general conclusion from the whole was, that the composition of sea waters in all parts of the ocean is very nearly the same, both with respect to the nature of the saline matters and their relative proportions to one another, and that they only differ from one another with respect to the absolute quantities of salt they contain.

In the course of the paper, the following interesting facts were mentioned. Dr. M. had been furnished with specimens of sea water from both the late arctic expeditions; and on comparing the labels attached to them, he found that in the Greenland seas the temperature, as ascertained by Lieutenants Franklin and Beechy, uniformly *increased* with the depth; while in Baffin's Bay, according to the observations of Capt. Ross and Lieut. Parry, it as constantly *diminished*. With respect to the composition of these waters, Dr. M. found that specimens taken from the surface were not generally less saline than those taken from great depths, unless the surface had been lately thawed, when the quantity of saline matter in the surface water was much diminished.

In speaking of the general composition of sea waters, Dr. M. stated the important discovery made by Dr. Wollaston that they uniformly contain *potash*. The proportion of this alkali present, Dr. W. estimates at somewhat less than  $\frac{1}{3000}$ th part of the water at its average density; and he supposes it exists in the state of sulphate.

#### SOCIETY FOR THE ENCOURAGEMENT OF ARTS, MANUFACTURES, AND COMMERCE.

This Society has voted the following rewards since April last:

*Mr. T. Hack's Chuck for a Lathe.*—This is somewhat similar to Mr. Bell's, before-mentioned; but it has four studs; and its construction is somewhat simplified. The Society voted its silver medal to Mr. Hack for this contrivance.

*Mr. J. Beckway's Hay-weigher and Binder.*—This instrument is made conveniently portable, and will be found exceedingly useful in adjusting the weight of trusses of hay, &c. They can also be more conveniently bound in this machine. The silver Isis medal and 15 guineas were awarded to Mr. Beckway for this invention.

*Dr. W. W. Thackeray's, Mr. Creyke's, and Mr. Palmer's Plantations of Timber Trees.*—These plantations are made upon land unfit for tillage; and the Society voted the gold medal to Dr. Thackeray, the silver medal to Mr. Creyke, and the silver Ceres medal to Mr. Palmer, for their valuable labours.

*Mr. T. Lane's Fruit-Gatherer.*—This useful instrument is affixed to a light pole of considerable length, and is made to grasp and hold the fruit firmly, but without injuring it. The silver Ceres medal, or 10 guineas, was adjudged to Mr. Lane for his invention.



*Mr. Edward Roberts's Churn.*—This churn has two agitators within it, and will be found to separate the butyraceous parts of the cream with great facility. The sum of five guineas was voted to Mr. Roberts for this contrivance.

*Mr. Aloys Senefelder's Treatise on Lithography.*—Mr. Senefelder was the original inventor of this valuable art, and his treatise contains the history and results of his discoveries with numerous recipes for making the inks, crayons, machines, and other apparatus, necessary in the process. The Society awarded its gold medal to Mr. Senefelder, as a testimony of its sense of his useful labours.

*Mr. D. Redman's Specimen of Lithography upon English Stone.*—The silver Isis medal was voted to Mr. Redman for this specimen.

*Mr. C. Hudmandel's Specimens of Lithography executed upon Foreign Stones.*—The Society voted its silver medal to Mr. Hudmandel, for his masterly performances in this art.

*Mr. Hinning's Restorations and Casts of the Friezes in the British Museum.*—The Society awarded its gold Isis medal to Mr. Hinning for this laborious performance.

*Mr. J. White's Double Door Spring.*—This invention is for the purpose of permitting the doors to open both ways, and yet to retain them when shut, with sufficient firmness. The Society adjudged its silver Isis medal to Mr. White for his invention.

*Lieut. T. Cook's Life-Raft.*—This valuable apparatus can be made in a very short time, from materials always on board ship, or it may be kept in a very portable form, always ready for use; and may prove the means of saving many lives in case of shipwreck. The Society voted its gold medal for this invention.

*Mr. Brabazon's Row-Lock.*—This is an improved mode of supporting the oars in boats, and particularly in life-boats; and for this contrivance, the Society awarded its silver medal.

*Mr. J. Young's British Opium.*—Mr. Young has considerably improved the method of collecting this valuable medicine; and has proved that it may be procured in this country with sufficient profit to induce the agriculturist to cultivate the poppy for this purpose, as well as to extract oil from its seeds. The Society adjudged its gold medal to Mr. Young for this improvement.

*Mr. Aust's Circular Pump.*—In this pump, the barrel and pump-rod are made circular, and the latter forms one piece with the handle, which, turning upon an axis in the centre of the circle, there is nothing to disturb the circular motion of the piston in the barrel, and the pump becomes exceedingly simple, consists of but few parts, and can consequently be made cheap. The sum of 20 guineas was voted to Mr. Aust for this contrivance.

*Mr. W. Russell's Lock-Cock.*—The handle of this cock cannot be turned, until a stud screwed into the side of it is unscrewed by means of a key applied to it; in some cases Mr. Russell applies a Bramah's lock to this purpose, and the cock, therefore,

becomes a very secure means of preventing the fraudulent waste of valuable liquors. The Society voted its silver medal.

*Mr. Ainger's Self-Adjusting Crane.*—This is a contrivance tending to equalize the power applied to work a crane according to the weight to be raised by it. The silver medal was awarded to Mr. Ainger.

#### ROYAL DANISH SOCIETY OF SCIENCES.

*View of the Transactions of the Royal Danish Society of Sciences, and of the Works of its Members, from May 31, 1817, to May 31, 1818.* By H. C. Ørsted, Professor, and Knight of the Cross of Dannebray.

During last year, the following gentlemen were elected foreign members :

*For the Mathematical Class.*—Mr. Wiebeking, of Munich, Privy-Councillor and Knight ; Mr. Flanti, of Naples, Professor.

*For the Physical Class.*—Mr. Gieseke, of Dublin, Commander of Dannebray ; Mr. Jameson, of Edinburgh, Professor.

When Kepler had discovered the law for the motion of the celestial bodies, viz. that the times in which given arches are run through are proportional to the planes inclosed between the said arches and the rays drawn to the centre of the powers, the problem of dividing a circle from a given point of the diameters in a given proportion naturally occurred to him ; for if this problem could be resolved with respect to the circle, the same must be the case with the ellipsis.

He proposed this question to the mathematicians, adding, that he did not believe it could be resolved in a direct manner ; and whoever could show him the contrary should be his magnus Apollo. The problem afterwards bore his name, and has since been considered by many mathematicians of the first rank. The discovery and progress of the analysis furnished the mathematicians with means enabling them to approximate more and more to an accurate solution of this celebrated problem.

Jeaurat had already set out the true anomaly by the middle anomaly unto the sixth power ; Cagnoli unto the ninth ; but as they merely gave results without any general formulas by which the explanation of the results thus found out might be continued at pleasure, no great progress was made as yet towards the real object.

It was reserved for the celebrated astronomer Schubert to remedy this defect. By explaining the terms for the functions of the eccentric anomaly, and by conveniently adapting it to the terms of the true one, this great analyst succeeded in giving a direct solution of Kepler's problem. In "Bode's Astronomisches Jahrbuch," for the year 1820, this solution is found, together with the numeral explanation for the true anomaly, and the radius vector, unto the 13th power of the eccentricity.

About the same time with Schubert, Professor Digin resolved to undertake this explanation unto the 16th power of the eccen-

tricity. Without making use of the eccentric anomaly, he explained the invaluable formula given by Laplace, in his "*Theory du Mouvement et de la Figure Elliptique des Planetes.*" This did not merely serve as a confirmation of the solution of Schubert, as the author found the numeral coefficients discovered by the latter to be quite right; but he also detected, by means of the observations made during the process, a very simple and symmetrical law for the explanation of the true anomaly, in consequence of which we may continue the same as far as we please; as also fix every term, independently of the other terms found out before. To these discoveries, the author added several explanations and applications useful in theoretical astronomy.

It is certain that the magnetical needle does not every where stand due north and south. In most places it declines considerably either towards the east or the west. This deviation, known at first only to seamen who made use of the magnetic needle to direct their course at sea, was afterwards found to lead to a knowledge of the spreading and diffusion of the magnetic powers over the globe, and may, perhaps, hereafter render the compass a still more perfect means of direction to the mariner than ever it could, had it every where, without variation, pointed to the same parts of the heavens. But if we desire, in this respect, to make the wished-for progress, the science must be continually enriched with observations respecting the deviations of the needle, which is perpetually changing in every place.

For London and Paris they have been so fortunate, with respect to the variations of the magnetic needle, as to have observations that extend further back than two and a half centuries, but the oldest of these cannot be considered as very exact.

With us the two Louises, father and son, have, through most part of the last century, made nice inquiries about the direction of the magnetic needle. Our present director of navigation, Mr. Wleugel, Commander and Knight, has, through a series of years, occupied himself with inquiries about magnetism, and has applied to this subject the additional knowledge and more perfect instruments with which he is favoured by the progress of the age.

In the remotest times, from which we have records of the magnetic needle, it had a declination towards the east, which gradually diminished, till about the middle of the 17th century it ceased in most parts of Europe; so that the needle stood regularly north and south, which soon was followed by a declination towards the west, that since that time has increased till very lately, when this westward declination again seems to be diminishing. But it is a matter of greater difficulty than it appears to be at first sight to determine whether this alteration has taken place or not. The declination of the magnetic needle is subject to incessant variations; every day is to it a period in which it increases and

diminishes ; every year the same alteration is repeated, but to a greater extent. As long as the daily declination is not too great in comparison with the yearly one, we may easily, after the lapse of a few years, be enabled to determine whether the deviation has increased or diminished ; but when the yearly alteration, as is now the case, is but small, when compared with the daily one, many years consequently will elapse before the amount of the yearly alterations will surmount that of the daily ones. That the yearly alteration is now become small is a circumstance which, no doubt, makes us believe that it has attained its maximum ; as every progressive series obtains its maximum when the difference of the terms becomes null.

During the year, the western deviation is greatest in the month of September ; and during the day it is greatest about two o'clock in the afternoon. When no considerable disturbances appear, the daily alteration does not exceed 20 minutes. In the year 1649, the deviation here in Copenhagen was  $1\frac{1}{4}^{\circ}$  east. About the year 1656, it must have been 0 ; as in 1672, it was  $3^{\circ} 35'$  west. The western declination afterwards continued to increase till the year 1806, when it was  $18^{\circ} 25'$ . Since that time it has diminished, however, as usual, advancing and relapsing. In the year 1817, Sept. 8, at two o'clock in the afternoon, it was  $17^{\circ} 56'$ , consequently  $29'$  smaller than in 1806 ; it may, therefore, be supposed, that the western declination has reached its maximum. By drawing the curve that is produced when the times are regarded as abscisses, and the declinations as ordinates, it seems to be evident that if the point of return does not fall upon the year 1806, it ought rather to be inquired for before than after that year.

The inclination of the magnetic needle has lately been found by the author to be  $17^{\circ} 26'$ .

Mr. Herhald, Professor and Knight, has delivered to the Society a description of a full-born fœtus, that expired during a complete breathing, about half an hour after its birth.

The internal organization of this fœtus was found to be in many respects rare and remarkable.

1. The viscera of the thorax and abdomen were arranged in an inverted order ; the inferior point of the heart and the arch of the arteria aorta turned to the right side ; the liver lay beneath the left, the spleen beneath the right, hypochondrium ; the broad part of the stomach, and the great arch of the same, bordered upon the spleen on the right side ; the duodenum began on the right side under the liver, and left its capsule before the spleen on the right side ; the pancreas turned its broad part to the left side, where its ductus ran into the duodenum ; the jejunum and ilium wound from the right side down towards the cœcum, which lay in the inferior part of the belly on the left side (*regio iliaca sinistra*) ; the colon wound about the small intestines from the left side towards the right, &c.



2. Both the ventricles of the heart were united by means of an opening in their septum ventriculorum, that ran from the ventriculus pulmonalis to the ventriculus aorticus; the arteria pulmonalis and ductus arteriosus Botalli to the aorta were wanting. Both venæ cavæ united in the breast before they went into the atrium of the heart. This atrium venarum cavarum received besides this from below on the left side, through the diaphragm, an extraordinary vein formed of the hepatic veins and umbilical vein, of a peculiar bulk. In the same atrium was an opening, that through the septum atriorum led into the atrium venarum pulmonarium, and another opening that led into the ventriculus aorticus. The atrium venarum pulmonarium had four openings before the venæ pulmonales, and one opening that led into the orificium venosum ventriculi pulmonalis; the venæ pulmonales were connected with an extraordinary branch of an artery, which from the inferior extremity of the arch of the aorta spread in the lungs; the aorta alone had transmitted all the blood from both the ventricles of the heart; the vena cava inferior went through the diaphragm on the right side, formed an arch in the concavity of the breast to the left side, to wind over the left bronchia, and then to join the upper and descending vena cava; the vena cava inferior received in the breast venæ intercostales from the right side, and vena hemiazygia from the left side, as otherwise the vena azygos was wanting. The upper arteria cœliaca did not give off any arteria hepatica; this organ received a peculiar artery from the arteria mesenterica superior.

The arteria cœliaca superior, and both arteriæ mesentericæ, ramified in an abnormalous direction corresponding with the inverted posture of the organs.

The arteria lienalis and arteria coronaria ventriculi ran to the right side; the arteria mesenterica superior wound with the ilio colica, and colica dextra to the left; the arteria mesenterica inferior turned to the right with its branches, colica sinistra, and hæmorrhoidalis interna, &c.

3. The organs for the secretion and ejection of the urine were also in an unusual condition; the membrum virile had its natural form and size, but the urethra was impervious from the top to the neck of the bladder. The kidneys were larger than in a natural condition, and divided into eight or nine bladders, formed like grapes, that contained a clear liquid. Every bladder had a small opening that led into the pelvis through a calix. Both ureters were widened, and particularly so near the vesica urinaria; they lay twisted like distended intestines on both sides of the abdomen, the breadth of which thereby was rendered unusual; the vesica urinaria, as also the ureters, were widened by the urine; the texture was of an uncommon thickness.

4. The child was besides what they call a varus; its feet were very much distorted; the sole of the foot turned inwards and backwards in such a manner that the toes touched each other.

The author illustrated all these monstrosities by drawings, and endeavoured, by adding an historical view of similar monsters, and by pointing out the different circumstances necessary for the life of the foetus and the new-born child, to show how important it is to the physiologist, the pathologist, the physician, and particularly to the forensic physician, to have a complete knowledge of the monsters in the animal economy.

As a continuation of this inquiry, the author delivered to the Society observations on the causes of organic monsters in general. He first observed, that the hypothesis of demoniacal and sodomitical foetus was formerly generally admitted to be true, but that all naturalists now look upon it as a dream from ancient times. He then showed that the doctrine of monsters from fantastical origin, as of the formation of the embryo, different from the general laws in consequence of the imagination of the mother, is as groundless as those ancient opinions, and has, in consequence, lost most part of its adherents. He himself looks upon the hypothesis, that it is possible for a woman big with child, by being frightened, or by mislooking herself, or by being desirous of some object or other, to be able to cause her embryo to be misshapen, by means of her imagination, as no less false and dangerous; though among the less enlightened part of mankind, it is generally embraced. He exhorts medical men to be zealous in checking the progress of this hypothesis, and with Mickel, Lawrence, and others, to study and explain those laws of nature that under different conditions determine the different forms of the embryos and all organic bodies.

Prof. Ørsted, Knight, presented to the Society two treatises, the one being the beginning of a series of treatises on the method after which a system in natural philosophy ought to be composed; the other containing an inquiry into the compressibility of water. Every one knows what a number of systematical books under the names of systems, compendiums, elements, courses, &c. in natural philosophy, have been published in different countries of Europe in a series of years. Although these, and even such as belong to one and the same age, contain many diversities of opinion respecting the causes of things, yet the diversities in the methods seem to be still greater. They have not even agreed with respect to the extent of the science. While some would debar from it as well what may be considered the application of mathematics to chemistry, others did not only want all this to belong to chemistry, but even added the doctrine of the condition of the terrestrial globe, and a view of the laws for the motion of the other planets. With respect to the division and the order of matters, there was no less disagreement, but in particular a great doubt seemed to prevail respecting the method after which those laws of nature, that were apt to be expressed mathematically, ought to be represented and proved in natural philosophy. All these objects produced a great many questions, to which the author will endeavour to reply in a series of trea-

tises, the object of the first of which is to explain what natural philosophy means. The author here endeavours to justify at large the definition he has made use of in other writings, according to which natural philosophy is the science of the general laws of nature; yet he will not confine this science within the narrow circuit which several German authors have drawn by the name of *algemeine naturlehre* (*physica generalis*); but admits into it the doctrine of electricity, magnetism, light, heat, and chemical affinities, as they are all immediate consequences of the general powers of nature. He is of opinion that even the qualities of the different matters from this more extended point of view ought to be regarded as peculiar effects of the general powers of nature, which in every one of them appear in a particular development and strength.

The author having already formerly endeavoured in several writings to prove that the electrical powers are the same with the chemical, only in a more free condition, and having at the same time alleged that magnetism, light, and heat, are effects of the very same powers, all in natural philosophy that does not treat of motion unite to form a coherent doctrine of powers or chemistry in the most extensive sense of the word. The first of these parts of natural philosophy (in this sense of the word) comprehends the external changes; the second the internal.

It is evident that no more can be added to these two parts, except the doctrine of the union of powers and motion; for instance, in light and the radiant heat; but whether this doctrine is to be separated from the rest as an independent chapter, or is to be comprised in the doctrine of powers, cannot perhaps be determined before natural philosophy has obtained a far greater perfection.

The author intends, as soon as possible, to give the continuation of these inquiries. His design is thereby to introduce and occasion an inquiry concerning this subject among natural philosophers, and thinks it very useful to the science if they could agree upon the form and construction of a system in the science; and all the learned in the said science would then work jointly to complete it; and thus in time a work would be produced containing a complete picture of the science in the age given. The author does not of course mean that all systematical books ought to be composed after the very same plan, which indeed might be altered according to the different aim of the author, but in such systems as aimed at nothing else but evidently to explain the science, he thinks that the same order and way of proceeding ought always to be followed, when the learned had previously agreed to declare it to be the proper one. But that such a concord respecting a theory of systematical books should be obtained, he thinks, to be as possible as the concord subsisting among the philosophers respecting so many other theories.

Although we are already possessed of such proofs of the com-



compressibility of water that it is not to be doubted of, yet the degree in which water can be compressed by a certain force and the law according to which the compression takes place are subject to uncertainties, and even very considerable ones. Some mathematicians and philosophers indeed were of opinion that the said compressibility of water was proportioned to the force employed; but the experiments of Abich and Zimmermann, which are the only series of experiments of the compressibility of water by unequal powers, seemed to be entirely contradictory to this opinion, wherefore Gehler, in the account which he has given in his dictionary of the experiments on the compressibility of water, plainly tells us that it has not been possible to discover any law in the quantities that occurs thereby. The same judgment has been passed by all other philosophers on the results of the experiments of Abich and Zimmermann.

Prof. Ørsted undertook to examine them anew, and was surprised at finding that the calculations of the results of the experiments, although of the easiest kind in Zimmermann's book, were perplexed by mistakes of consequence. By correcting them it appeared that the experiments described did not prove that unboiled water is less compressible than boiled, but that, on the contrary, as might be expected, it was even more compressible. It was likewise demonstrated that the compressions, so far as the experiments were not undertaken with the greatest power, which, perhaps, might compress the piston itself, were proportioned to the compressing powers. This induced the author to get a machine made for the compression of water. It consisted of a very wide and thick brass cylinder, and a thin tube with a piston for the compression of the water. By means of this machine, the water was compressed by applying but very little force, and very small alterations in the place filled with the water were measured. In order to measure or compute the compressing power, which cannot be done immediately with sufficient exactness on account of the friction of the piston, an appendage was made to the wide cylinder into which a narrow and strong glass tube was screwed. This was filled with air, which by the greatness of its compressibility showed, after the law of Mariotte, the quantity of the pressing power. By this instrument he found that the compression of the water was proportional to that of the air, consequently in proportion with the compressing powers. At 12° of the centigrade thermometer this compression differs very little from 0.00012 for a pressure equal to that of the atmosphere. He has likewise extended his criticisms to Canton's experiments, and has shown that a just estimation of the influx of heat into the water would give a greater compressibility than that which the English philosopher has concluded. As De La Place has founded his calculations of the swiftness of the sound in water upon the experiments of Canton, the result of his calculation must be adjusted after the later experiments.

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The reason of this is, that the vena lumbaris on entering the pelvis, forms two ramifications, the middlemost of which joins the vena cava; the uppermost goes immediately into the kidneys; while the nethermost, together with the sciatic vein and branches of the vena ossis sacri forms the vena renalis inferior, which, for the most part, lies buried in the substance of the kidneys.

The peculiar organ which the author has found in the reptiles consists of a serous membrane with many veins. This membrane is formed like a bladder; and in those reptiles whose skins are without scale, it is united with the great gut, but in reptiles covered with scales, the organ is not thus united, but is filled with fat. The author does not believe that this difference prevents us from concluding that this organ is the same in all these animals, and supposes that the difference is only to be ascribed to the skin's unequal aptness of respiration. This is, however, by the committee of the Society, looked upon as dubious.

After all this, the blood that proceeds from the hinder part divides into two parts; the first of which is led through the foremost vena abdominalis to the liver, where it associates with the vena portæ, and thus contributes to prepare the gall; the other part, that proceeds from the sciatic vein and vena caudalis, goes to the kidney; and in the author's opinion serves to the secretion of the urine. That the blood from the hinder part of the body goes to the kidneys, as has been alleged, the author proves as follow: *a.* That the vena renalis inferior, in all the animals here mentioned examined by him, are connected with the sciatic vein and the vena caudalis. *b.* That the said vena renalis grows larger in proportion as it approaches the kidney, and as it proceeds receives more branches, whereas its greatness again abates in proportion as it forms ramifications to the kidney. *c.* That it ramifies so nicely in the kidney that immediate union with the other venæ renales superiores and the vena cava can be discovered. *d.* That even in those animals in which the connexions of the vena abdominalis and the vena renalis inferior is most exact, you may, by compressing the vena abdominalis, hinder the blood from proceeding from the hinder extremities to the liver without occasioning it to be stopped in the vena sciatica and vena lumbaris; whence it is evident that it must flow off through the ramifications of the inferior vena renalis in the kidney. *e.* Lastly, that there are animals, for instance, the snake, in which the vena abdominalis anterior and the vena renalis inferior are entirely separated, so that the blood from the hinder parts of the body in these animals cannot flow but through the vena renalis. That the blood thus led to the kidney serves to secrete the urine, the author endeavours to prove from the spreading and nice ramifications of these veins in the kidneys; from the smallness of the arteria renalis that appears to be insufficient to the secretion of the urine in these animals; and from

the size of the veins thereto adapted. Add to this, that the veins that go to the liver give an example of such an action in the veins by secreting the gall.

The effect thus ascribed to the vena renalis, the committee of the Society, however, found to be dubious, as from the smallness of the arteria renalis no conclusion can be made of their insufficiency to the secretion of the urine, without knowing the quantity of the urine in the different animals; as from the size of an organ of secretion, it is not always possible to conclude how great a quantity it can secrete; as the secretion of the urine in man and mammalia is performed by arteries; while that of the gall is performed by veins; and, lastly, as the nature of the urine and of the gall are quite contrary to one another, the latter being extremely combustible, the former extremely oxygenous, it is not at all likely their origin should be very much like one another.

As for the rest, the committee approved the treatise, and found that the author, by the anatomical inquiries and experiments therein delivered, had partly rectified, partly enriched zoology, and thus opened a wide prospect for views that perhaps might be of importance to the pathology of man.

In consequence of this declaration of the committee, the Society resolved to receive the treatise delivered of Professor Jacobsen among its writings.

In a treatise sent to the Society, intelligence was given of the catching of a certain sea-animal, called *grind*, together with an addition to the natural history of the *grind*.

The author, who, in 1817, undertook a botanic journey to the Feroes, happened, during his stay in these islands, to witness the catching of a grind; and as, on this occasion, he took down several observations relating to the natural history of this animal, and instantly made a sketch of it, he was thereby enabled to deliver a more complete and exact information than had hitherto existed of this animal, so important for the inhabitants of Feroe. The grind has indeed been mentioned by several authors; for instance, Debes, Saabo, and Sandt, and cannot, therefore, be thought to be unknown; but on account of the incomplete and partly erroneous description we have hitherto had, it has not been possible for systematic writers to give this animal its proper place; wherefore some have thought it belonged to the genus *delphinus*, others to *balæna*. After having given an account of the method of catching the grind used at Feroe, the author, by a methodic description, as circumstantial as could be obtained, and by adding a drawing, has proved that the grind belongs to the genus *delphinus*; and as it has not been admitted into the *systema naturæ*, he proposes to call it *delphinus grinda*. This animal being of great importance to the inhabitants of Feroe, as the wealth or poverty of these islanders in a great measure depends upon the more or less successful catching of it, it ought

to be more known than hitherto. This animal, like many other kinds of fishes as well as birds, is very social; and is, therefore, always found in flocks of 100 or 1000. When the inhabitants of Feroe at fishing happen to observe such a flock, as they commonly tumble about on the surface of the water, the seamen endeavour to drive them into a sandy bay, nor are they unwilling to suffer themselves to be driven before the boat; and by the assistance of more boats hastening to help, they are driven ashore in a hurry, and are killed with lances or spears made for that purpose. In the summer of the year 1817, the inhabitants of Feroe in this manner caught in different flocks more than 600 grinds. This species of grind, or delphinus, which is six or 10 ells long, is not only found about the islands of Feroe, but also about Iceland and the Orader; in the latter place it is caught in the same manner as on the Feroes, and is there called the ca'ing whale. Besides this kind, there is yet another smaller species of delphinus at the Feroes, which they there call bovhvidehval, and which, partly by an erect fin on the back, partly by a snow-white belly, and other singularities, distinguishes itself from the aforementioned; but as this species seldom appears, and was not caught while the author was on the islands, he has not been able to give any sufficient information of it; but supposes from what the inhabitants told him, that this kind constitutes a singular and new species.

The Society found this treatise to be worthy of being admitted into its writings, and presented the author with its silver medal as a mark of esteem.

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## ARTICLE X.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Specific Gravity of Hydrogen Gas.*

A very careful set of experiments was made a few weeks ago in my laboratory to determine the specific gravity of pure hydrogen gas. A quantity of zinc was distilled in a stone-ware retort in order to have that metal in a state of complete purity. The zinc thus purified was dissolved in dilute sulphuric acid, which had been in the first place carefully distilled in a glass retort, and afterwards diluted with distilled water. The hydrogen disengaged was collected in glass jars. Its specific gravity was taken in the usual manner by means of a large glass flask, which was weighed while full of air, exhausted, and again weighed, and finally filled with hydrogen gas, and weighed a third time. The weight of the exhausted flask subtracted from that of the flask



full of air gave the weight of the common air displaced by the exhaustion =  $a$ . The weight of the exhausted flask subtracted from that of the flask when full of hydrogen gas gave the weight of the hydrogen gas introduced into the flask =  $b$ . It is obvious that the specific gravity of the hydrogen gas is  $\frac{b}{a}$ . Three successive experiments were made, which gave each the same result. We found the specific gravity of the hydrogen gas to be 0.06933. Thus these experiments confirm the conclusions deduced by Dr. Prout from the specific gravity of ammoniacal gas, and likewise the experimental conclusions of Berzelius and Dulong, mentioned in the number of the *Annals of Philosophy* for May last.

In a former set of experiments which I made some years ago, I found the specific gravity of hydrogen gas 0.073; but my apparatus was not so delicate, and I did not take the precaution to distil the zinc which I employed.

## II. Ore of Tellurium.

Our mineralogical readers will be gratified to learn that the ore of this very scarce metal has been recently discovered in Connecticut. It occurs in a bismuth mine belonging to Mr. Ephraim Lane, a farmer, at New Stratford, town of Huntington, Connecticut. This mine has been sunk only to the depth of 10 feet. It affords native bismuth, native silver, magnetical and common iron pyrites, copper pyrites (in crystals), galena, blende, tungsten, tellurium, &c.

In the third number of Dr. Silliman's *American Journal of Science*, in which this discovery is announced, nothing is said respecting the state in which the tellurium exists in this mine. But it is stated that letters, post paid, addressed as above, will find Mr. Lane, who will, for a reasonable compensation, pack boxes, more or less extensive, for mineralogists and others.

## III. Native Copper.

In Bruce's *Mineralogical Journal* (i. 149), mention is made of a remarkable piece of native copper found near New Haven, in Connecticut, many years ago, weighing about 90 lbs. Dr. Silliman has announced in the first number of his scientific journal (p. 55), that another piece has been recently found half a mile west of the Hartford turnpike road, opposite the town of Wallingford, and 12 miles from New Haven. It was turned up in ploughing to repair a road. The country is of the secondary trap formation, and the rocks at the particular place are the old red sandstone of Werner, which here occupies the plains, and runs under the trap. The piece weighs almost 6 lbs. It is beautiful virgin copper, with rudiments of large octahedral crystals of native copper upon its surface, which is more or less incrustated with green carbonate of copper, and ruby oxide, very much

resembling that of Cornwall. The ruby oxide is particularly remarkable in the cavities of the piece.

#### IV. *Fibrous Prehnite.*

The variety of prehnite which occurs so abundantly in the neighbourhood of Glasgow, is of the fibrous kind. Its specific gravity is greater than that of foliated prehnite, being 2.901. It does not break so readily as the foliated variety. I have not been able to satisfy myself whether it be harder. Its colour is apple-green, and it is composed of fibres which radiate from several different centres. I had the curiosity to analyze a specimen of it, in order to compare it with the foliated variety, the only one hitherto, I believe, subjected to analysis. The result was as follows :

Silica . . . . .	43.60
Alumina. . . . .	23.00
Lime . . . . .	22.33
Oxide of iron. . . . .	2.00
Water. . . . .	6.40
Loss . . . . .	2.67
	<hr/>
	100.00

This result approaches very nearly that obtained by Gehlen from a variety of prehnite from Ratschinke. He does not describe the specimen ; but from its specific gravity, 2.924, I think it probable that it was similar to our Glasgow prehnite, or that it belonged to the fibrous variety. Its constituents, as he obtained them, were as follows :

Silica . . . . .	43.00
Alumina. . . . .	23.25
Lime . . . . .	26.00
Oxide of iron. . . . .	2.00
Oxide of manganese . . . . .	0.25
Magnesia, a trace. . . . .	—
Volatile matter . . . . .	4.00
Loss . . . . .	1.50
	<hr/>
	100.00 *

It seems probable, from the preceding analysis, which must be nearly exact, that fibrous prehnite is a compound of one atom of silicate of alumina and one atom of bisilicate of lime. Its symbol, therefore, will be  $A / S + C S^2$ . Foliated prehnite seems to contain a greater proportion of alumina, and a smaller of lime, at least if Klaproth's analysis be considered as accurate.

\* Schweigger's Journal, iii. 186.

V. *Necronite* (a supposed new Mineral).\*

*Extract of a Letter from Dr. H. H. Hayden, of Baltimore, to Dr. Silliman, dated Jan. 5, 1819.*

"It (the necronite) occurs in a primitive marble, or limestone, which is obtained 21 miles from Baltimore, and a small distance from the York and Lancaster road. It was first noticed by myself at Washington's monument, in which this marble is principally employed.

"It occurs, for the most part, in isolated masses in the blocks, or slabs, both in an amorphous and crystallized state. It is most commonly associated with a beautiful brown mica, of the colour of titanium; small but regular crystals of sulphuret of iron, tremolite, and small prismatic crystals of titanium, which are rare. The form of the crystals is a rhomboid, approximating very much to that of the felspar, and which has inclined some to consider it as such. Also, the hexaedral prism, resembling that of the beryl. This form is rare, and has not as yet, I believe, been found complete. Its colour is a bluish white, and clear white. Its structure much resembles felspar, being lamellar; sometimes opaque, semi-transparent and transparent, at least in moderately thin pieces. It scratches glass, carbonate of lime, and even felspar, in a *slight* degree. In all our efforts, it has been found infusible, per se, or with borate of soda; and even from all the force of heat that could be excited in a smith's furnace, it came out unchanged in any degree. The acids seem to have no sensible effect upon it, either cold or hot. This is all that I can say of it at present, except that it possesses a most *horrid* smell.† I have since found in a marble of the same kind, but from a different quarry, and a few miles distant from the first, a quartz almost as fetid as the necronite, and likewise associated with *small* prisms of titanium.

"These substances carry with them a degree of interest in another point of view. They seem to invalidate the opinion that the fetid smell of secondary limestone, slate, &c. is derived from the decomposition of animal matter, as their gangue is *decidedly* a rock of primitive formation."

*Another new Mineral observed by Dr. Hayden.*

"Exclusive of the interest which the necronite has excited with me and several others, I have besides stumbled upon another substance, if possible, still more interesting. I discovered it in an imperfect state, about four years since, but not until recently have I been able to find it perfect, in beautiful garnet-coloured cubic crystals one quarter of an inch square, or nearly. These crystals are very liable or subject to decomposition, in which state they present a perfect but spongy cube.

\* From Silliman's *American Journal of Science*, i. 306.

† On account of its peculiar cadaverous odour, Dr. Hayden proposes to call this mineral (should it prove to be a new one) *Necronite*, from the Greek *νεκρος*.

Although they resemble the cubic zeolite, yet they have nothing of its character with them besides."

#### VI. Native Carbonate of Magnesia.

This mineral species was discovered a good many years ago by Dr. Mitchell, of Belfast, in a serpentine rock at Hrubtschitz, in Moravia. It has been recently discovered by Mr. James Peirce, at Hoboken, in Staten Island, on the New Jersey side of the Hudson, the very same place where the hydrate of magnesia was discovered by Dr. Bruce. The carbonate of magnesia occurs in a serpentine rock, in horizontal veins, about two inches in thickness. When first taken out, it was soft, white, and very slightly adhesive from a little moisture; but when dry it fell to powder with friction. It was totally soluble in sulphuric acid with effervescence, and yielded crystals of Epsom salt. Mr. Peirce did not ascertain the proportion of carbonic acid which it contained; but from Bucholz's analysis of the native carbonate of magnesia found in Germany, we learn that it is a compound of one atom carbonic acid and one atom of magnesia; or by weight of

Carbonic acid. ....	2.75	.....	52.38
Magnesia .....	2.50	.....	47.62
	<u>5.25</u>		<u>100.00</u>

There can be little doubt that the composition of the American carbonate of magnesia will be similar.

More lately Mr. Peirce discovered in the same place veins of native carbonate of magnesia in fine acicular crystals. The discovery was made during the examination of an excavation dug about three miles from the Quarantine under the delusive expectation of finding gold. The native carbonate of magnesia was observed in very white acicular crystals, grouped in minute fibres, radiating from the sides; but not always filling the veins and cavities in which it was found. The crystals were in some instances suspended, assuming a stalactitical form.—(Silliman's Journal, i. 54 and 142.)

#### VII. Picromel.

This is a name given by Thenard to the peculiar substance to which ox bile is indebted for its properties. Berzelius first pointed out an easy method of procuring it. Sulphuric acid throws it down in the state of a green resinous-looking matter, which used formerly to be denominated *resin of bile*. If we put this matter along with a quantity of carbonate of barytes in powder into a flask or retort containing a sufficient quantity of distilled water, and place the vessel upon a sand-bath, the sulphuric acid gradually unites with the barytes, and the picromel being set at liberty dissolves in the water. We have only to





VIII. *Additional Facts respecting Gauze Veils as a Preservative from Contagion.* By Mr. Bartlett.

(To Dr. Thomson.)

Buckingham, May 3, 1819.

SIR,

Perceiving that my letter to you respecting *gauze veils* as preventives of contagion has excited some attention by being copied into the public journals, I beg leave to express a hope that in case any of your scientific readers, or correspondents, should put it to the test of experiment, they will communicate the result to the public through the pages of your *Annals*.

I think it right to state, that a child of mine, aged 15 months, has escaped a very prevalent (and, what in this neighbourhood has recently proved, fatal) disorder, the *hooping-cough*, notwithstanding she has almost daily associated with children labouring under its effects; and which I can only attribute to the circumstance of her having constantly worn a common green *gauze veil*. This, I grant, as a solitary instance, is but a very slender foundation to support any thing like an hypothesis upon; but in contributing, with others, my *mite* of information, a connected chain of concurring testimony may at length be formed, and the *bar* removed which has so often deprived the afflicted sufferer of the last sad consolation of a communion with those who by friendship or affection had, perhaps, rendered existence valuable; whilst the intercourse betwixt man and man (so often interrupted by pestilence) may be renewed in confidence and security.

It has been doubted whether there be such a thing as *infection*; all prevalent diseases being deemed epidemical, arising from animal, or other effluvia;\* but it is immaterial to the present question, whether this opinion acquire credit or not, since the pestilential virus (according to either doctrine) must be introduced into the system by means of the respiratory organs, and not by immediate contact. The fact is, the cause of numerous disorders remains undiscovered; and until experience proves that miasmata may become incorporated with the animal fluids through any other channel than that of respiration, it is fair to reason upon any probable hypothesis. I have the honour to be,

Your very obedient servant,

J. M. BARTLETT.

IX. *Respiration of Oxygen Gas.*

The following paragraph is copied from the first number of Dr. Silliman's *American Journal of Science*, p. 95:

"It is not extraordinary, when oxygen gas was first discovered, and found to be the principle of life to the whole animal creation, that extravagant expectations should have been formed

\* The subject has lately been brought before the House of Commons, but the theory is by no means new.

as to its medicinal application. Disappointment followed of course, and naturally led to a neglect of the subject; and, in fact, for some years, pneumatic medicine has gone into discredit, and public opinion has vibrated to the extreme of incredulity. Partaking in a degree in this feeling, we listened with some reluctance to a very pressing application on this subject during the last summer. A young lady, apparently in the last stages of decline, and supposed to be affected with hydrothorax, was pronounced beyond the reach of ordinary medical aid. As she was in a remote town in Connecticut, where no facilities existed towards the attainment of the object, we felt no confidence that, even if oxygen gas were possessed of any efficacy in such cases, it would *actually* be applied, in this case, in such a manner as to do any good. Yielding, however, to the anxious wishes of friends, we furnished drawings for such an apparatus as might be presumed attainable, and also written and minute directions for preparing, trying, and administering the gas. It was obtained from nitrate of potash (saltpetre), not because it was the best process, but because the substance could be obtained in the place, and because a common fire would serve for its extrication. The gas obtained had, of course, a variable mixture of nitrogen or azote, and probably on an average might not be purer than nearly the *reversed* proportions of the atmosphere; that is, 70 to 80 per cent. of oxygen to 20 or 30 nitrogen; and it is worthy of observation, whether this circumstance might not have influenced the result.

Contrary to our expectations, the gas (as we are since informed by good authority) was skilfully prepared and perseveringly used. From the first, the difficulty of breathing and other oppressive affections were relieved: the young lady grew rapidly better, and in a few weeks entirely recovered her health. A respectable physician, conversant with the case, states, in a letter now before us, "that the inhaling of the oxygen gas relieved the difficulty of breathing, increased the operation of diuretics, and *has effected her cure*. Whether her disease was hydrothorax, or an anasarous affection of the lungs, is a matter I believe not settled."

Should the revival of the experiments on the respiration of oxygen gas appear to be desired, it would not be difficult to simplify the apparatus and operations so as to bring them within the reach of an intelligent person, even although ignorant of chemistry: and this task, should there be occasion, we would cheerfully undertake to perform.

This interesting class of experiments ought to be resumed, not with the spirit of quackery, or of extravagant expectation, but with the sobriety of philosophical research; and it is more than probable that the nitrous oxide, which is now little more than a subject of merriment and wonder, if properly diluted and discreetly applied, would be productive of valuable effects."

X. *Some Corrections and Additions to Mr. Rice's Paper on the Weight of a Cubic Inch of distilled Water, contained in the Number for May of the Annals of Philosophy.* By Mr. Rice.

(To Dr. Thomson.)

SIR,

Dublin, May 10, 1819.

In my paper "On the Weight of a Cubic Inch of Distilled Water, and the Specific Gravity of Atmospheric Air"; inserted in the *Annals of Philosophy* for this month, I find that, in computing the weight of a cubic inch of water from Sir George Shuckburgh's experiments, I have committed a mistake, which, though trifling in its consequence, demands particular notice. You will do me a great favour by giving a place to the following errata and observations, if possible, in your next number.

I am, Sir, your obedient servant,

E. S. M. RICE.

Page 342, line 12, for  $64^{\circ}$  read  $66.4^{\circ}$ ; line 26, for actually less value, read actually greater value; line 29, for subtracted, read added.

In consequence of these alterations, it is evident that a few of the subsequent numbers will require correction.

The signs prefixed to the numbers indicating "the corrections to be made for error from buoyancy," should be *positive* instead of *negative*: hence the mean deduced in line 10, should be 28722.4611; line 16, for 28672.7427, read 28672.8958; line 17, the same; also for 252.580, read 252.582; line 28, for 252.529, read 252.531. A bare inspection will show the alterations to be made in the other numbers.

*On the Proportion of Oxygen in Air.*—Estimating the specific gravities of oxygen and nitrogen in relation to air, as 1.1111 and 0.9722 : 1, and air to water as 0.0012085 : 1, we find atmospheric air composed of four volumes nitrogen and one volume oxygen, or

$$\begin{array}{r} 0.8 \text{ volumes nitrogen } 0.0002685 \\ 0.2 \text{ volumes oxygen } 0.0009400 \\ \hline 1.0 \qquad \qquad \qquad 0.0012085 \end{array}$$

Under the idea of this being the true composition, Dr. Prout adopted the specific gravities for oxygen as here given; subsequent experiments have confirmed its accuracy; and I think the trials hitherto made by the voltaic eudiometer cannot be considered as sufficiently precise to disprove the supposition on which it was calculated.

Chaptal, in his *Chemistry*, iii. 210, English translation, from analyses of air by detonation with hydrogen, rates the oxygen at 20 per cent. Mr. Dalton mixed 100 volumes of air with 60 volumes of hydrogen, and fired by the electric spark, the absorption indicated the presence of 20 volumes oxygen; the residue contained none.—(Nicholson's *Journal*, xiii. 434.)





the subject of the chemical changes which take place in fruits, they repropose the question in the following detailed manner:

To determine the chemical changes which take place in fruits during their progress to, and after their arrival at, maturity.

For the solution of this question, it is necessary, first, to analyze the fruits at different periods of their growth and maturity, and also when they begin to decay, and are completely rotten. Secondly, to compare the nature and quantity of the principles contained in them at these different periods. Thirdly, to examine with care the influence of external agents, particularly that of the air in contact with the fruit, and to ascertain the changes, if any, which it undergoes. The observations may be confined to certain fruits of different species, provided that it be possible to draw from them conclusions sufficiently general.

The second subject proposed by the Academy is,

To give a comparative description of the brain in the four classes of vertebral animals, and particularly in the reptiles and fishes; by endeavouring to trace the analogy of the different parts of that organ, by observing the changes of form and proportion which they display, and by following, as deeply as possible, the branches of the cerebral nerves. It will be sufficient to confine the observations to a certain number of genera selected from the principal natural families of each class of animals, though it will be necessary that the principal preparations be represented by drawings sufficiently detailed to admit of their being made over again, and thus be accurately verified.

The prizes for both these subjects will be a medal of the value of 3000 francs. The different memoirs must be transmitted to the secretary, free of expense, by Jan. 1, 1821; and the prizes will be adjudged at the public meeting in the month of March following. Each memoir must be marked by a motto, or devise, and accompanied by a sealed letter, containing the same motto, or device, and the name of the author.

The Academy also offer a gold medal of the value of 440 francs (given by an anonymous individual) for the work printed, or in MS. which shall be transmitted to them before Dec. 1, 1819, and which shall appear to them to contribute most to the progress of experimental physiology. This prize will be adjudged at the public meeting in March, 1820; and the candidates are informed, that the Academy will not return the different memoirs; but the authors are at liberty to take copies of them if they choose.

#### XIV. *Natural History of the Moluccas.*

Mr. H. Kuhl, a gentleman eminently qualified for the subject, is about to depart for the Moluccas, to explore the natural history of these interesting islands.

## ARTICLE XI.

*Magnetical, and Meteorological Observations.*  
By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*

Latitude  $51^{\circ} 37' 43''$  North. Longitude West in time  $1^{\circ} 20' 1''$ .

*Magnetical Observations, 1819. — Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
May 1	8 <sup>h</sup> 35'	24° 31' 09"	1 <sup>h</sup> 25'	24° 43' 04"	7 <sup>h</sup> 10'	24° 31' 23"
2	8 35	24 31 46	1 35	24 41 00	7 10	24 35 26
3	8 40	24 33 18	1 25	24 42 00	7 10	24 34 29
4	8 40	24 33 49	1 15	24 40 53	—	—
5	8 40	24 32 38	1 20	24 40 41	7 15	24 36 13
6	8 25	24 36 14	—	—	7 15	24 35 45
7	8 35	24 32 44	1 20	24 41 47	7 05	24 35 32
8	8 35	24 32 58	1 25	24 40 20	7 20	24 36 24
9	8 40	24 34 09	1 15	24 42 53	7 35	24 28 29
10	8 25	24 31 44	—	—	7 20	24 34 02
11	8 45	24 33 39	1 15	24 39 51	7 20	24 34 02
12	8 40	24 32 43	1 25	24 40 58	7 30	24 35 57
13	8 35	24 32 23	1 25	24 40 18	—	—
14	8 35	24 32 39	1 25	24 41 26	7 30	24 37 44
15	8 35	24 33 28	1 25	24 44 04	7 30	24 34 40
16	8 35	24 33 20	1 40	24 39 26	7 35	24 34 46
17	8 35	24 33 38	1 20	24 40 58	7 30	24 34 41
18	8 35	24 32 15	1 25	24 40 29	7 30	24 36 16
19	—	—	1 20	24 41 12	—	—
20	8 20	24 39 00	—	—	—	—
21	8 40	24 31 43	1 15	24 42 19	—	—
22	8 40	24 32 32	1 25	24 43 02	7 35	24 39 52
23	8 40	24 30 20	1 40	24 42 00	—	—
24	8 50	24 33 12	1 20	24 42 28	7 20	24 35 06
25	8 40	24 30 45	1 30	24 41 59	7 35	24 33 19
26	8 40	24 31 30	—	—	7 35	24 33 03
27	8 40	24 29 24	1 25	24 41 34	—	—
28	8 35	24 30 32	1 20	24 39 46	7 35	24 33 54
29	8 45	24 30 30	1 20	24 40 25	7 40	24 35 13
30	8 45	24 33 58	1 25	24 42 06	7 35	24 34 43
31	8 35	24 34 24	1 20	24 39 53	7 35	24 36 06
Mean for Month.	8 37	24 32 42	1 24	24 41 22	7 26	24 34 10

## Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
May	Morn....	29.364	50°	36°	S by W		Very fine	40°
1	Noon....	29.365	60	27	S		Very fine	60½
	Even....	29.355	54	30	S by W		Cloudy	
	Morn....	29.275	55	31	SE		Hazy	45
2	Noon....	29.250	65	23	ESE		Fine	67
	Even....	29.230	56	27	E		Cloudy	
	Morn....	29.160	61	29	E by S		Cloudy	52
3	Noon....	29.170	66	24	E by S		Cloudy	70
	Even....	29.148	60	30	ENE		Cloudy	
	Morn....	29.048	55	61	E		Rain	53
4	Noon....	29.080	61	47	E by S		Fine	63
	Even....	—	—	—	—		—	
	Morn....	29.123	55	62	SSE		Cloudy	49
5	Noon....	29.178	62	41	S by W		Fine	64
	Even....	29.267	56	46	SW by W		Very fine	
	Morn....	29.469	54	55	SW		Fine	46½
6	Noon....	—	—	—	—		—	65
	Even....	29.558	56	42	S		Fine	
	Morn....	29.585	56	46	E by S		Fine	47½
7	Noon....	29.600	63	38	ESE		Fine	65½
	Even....	29.600	57	43	E		Cloudy	
	Morn....	29.576	58	42	E		Fine	52
8	Noon....	29.600	68	30	ESE		Fine	68
	Even....	29.600	58	31	E		Fine	
	Morn....	29.660	60	39	Var.		Very fine	50
9	Noon....	29.673	69	28	SSW		Very fine	71
	Even....	29.680	58	55	NNE		Showery	
	Morn....	29.740	56	57	NNW		Very fine	49
10	Noon....	—	—	—	—		—	66½
	Even....	29.720	60	43	WNW		Cloudy	
	Morn....	29.649	56	57	W by N		Cloudy	52½
11	Noon....	29.700	61	42	W		Cloudy	
	Even....	29.680	59	50	WNW		Cloudy	61½
	Morn....	29.650	60	45	W		Cloudy	54
12	Noon....	29.645	64	35	W by S		Cloudy	64½
	Even....	29.600	58	47	W by N		Cloudy	
	Morn....	29.606	56	47	W by N		Fine	50
13	Noon....	29.608	61	37	NW by N		Cloudy	61
	Even....	—	—	—	—		—	
	Morn....	29.681	53	46	NNW		Very fine	44½
14	Noon....	29.691	60	36	NW by N		Fine	61
	Even....	29.650	55	42	NNW		Very fine	
	Morn....	29.608	53	48	NE		Very fine	44
15	Noon....	29.584	62	35	Var.		Cloudy	65½
	Even....	29.563	54	50	ESE		Cloudy	
	Morn....	29.594	55	45	SSE		Very fine	45
16	Noon....	29.591	66	33	SSE		Fine	67
	Even....	29.570	53	37	S		Cloudy	
	Morn....	29.524	58	41	SE		Fine	47½
17	Noon....	29.480	67	28	Var.		Fine	69
	Even....	29.400	59	31	SSE		Fine	
	Morn....	29.333	58	46	SW		Fine	49½
18	Noon....	29.352	66	39	SSW		Showery	66½
	Even....	29.352	58	43	SSW		Cloudy	





## ARTICLE XII.

## METEOROLOGICAL TABLE.

1819.	Wind.	BAROMETER.			THERMOMETER.			Hygr. at 9 a. m.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
4th Mon.									
April 17	S	29.57	29.40	29.485	55	43	49.0	75	15
18	S W	29.76	29.57	29.665	58	36	47.0	62	4
19	W	29.76	29.72	29.740	57	47	52.0	64	44
20	S W	29.72	29.59	29.655	61	46	53.5	82	—
21	S W	29.79	29.59	29.690	59	42	50.5	72	—
22	N W	29.85	29.74	29.795	49	42	45.5	76	—
23	E	29.74	29.51	29.625	51	46	48.5	90	15
24	E	29.65	29.51	29.580	52	45	48.5	100	25
25	N E	30.08	29.65	29.865	50	34	42.0	76	2
26	E	30.16	30.08	30.120	52	32	42.0	68	—
27	E	30.16	—	—	56	25	40.5	62	—
28	S E	—	30.05	30.105	60	31	45.5	60	—
29	E	30.05	29.80	29.925	59	28	43.5	60	—
30	S E	29.80	29.75	29.775	60	28	44.0	60	—
5th Mon.									
May 1	S W	29.75	29.68	29.715	66	33	49.5	57	—
2	S E	29.68	29.57	29.625	69	48	58.5	52	—
3	S E	29.57	29.45	29.510	71	49	60.0	58	—
4	S E	29.50	29.45	29.475	69	50	59.5	87	68
5	S E	29.88	29.50	29.690	65	44	54.5	69	—
6	W	30.00	29.88	29.940	64	39	51.5	62	—
7	S E	30.00	29.98	29.990	66	49	57.5	67	—
8	E	30.17	29.98	30.075	69	44	56.5	63	—
9	S E	30.14	30.06	30.100	73	46	59.5	59	18
10	N W	30.14	30.10	30.120	69	53	61.0	65	—
11	W	30.10	30.04	30.070	65	54	59.5	67	—
12	N W	30.10	30.04	30.070	67	53	60.0	61	—
13	N W	30.10	30.00	30.050	64	41	52.5	60	—
14	N W	30.04	30.02	30.030	64	40	52.0	58	—
15	N	—	—	—	67	42	54.5	59	—
		30.17	29.40	29.831	73	25	51.67	67	1.91

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Fourth Month.*—17. Much wind in gusts, a. m.: the clouds large, and carried at a great elevation: the first swallows appeared: wet squalls, p. m.: some lightning about 10, and a gale through the night. 18. *Cumulus*, with the lighter modifications above, followed by *Nimbi* and wind: hail in a shower about three, p. m.: the rainbow twice: calm at evening. 19—22. Mostly cloudy, with *Cumulostratus*: the cuckoo was heard in this interval. 23. Gloomy sky, with much *Cirrostratus* at evening: rain in the night. 24. Drizzling, a. m.: wet and windy, p. m. 25. A gale, with much cloud in the morning: fair, p. m. 26. Fair, with heavy *Cumulostratus*. 27, 28. Hoar-frost: clear, fine days, with *Cumulus*, *Cumulostratus*, and *Cirrus*. The dark part of the moon's disk, which has been scarcely discernible this winter is again plainly visible by day, as she follows the sun. 29. Hoar-frost: fine day, with *Cirrus* and breezes.

*Fifth Month.*—1. Fine: much *Cirrocumulus*, mixed with *Cirrus*: the wind a breeze. The gardens have suffered a little by the late frosts, more especially among the gooseberry bushes, which have cast about half their crop with us. 2. A superior westerly current appeared, a. m. carrying flocks of *Cirrocumulus*: between this and the S.E. wind below were large plumose *Cirri*, on one of which appeared a trace of a solar halo. These clouds increasing, with haze and *Cumuli* intermixed, the character of the sky became electrical: there was a lunar corona and a small bright halo at night with some lightning to the S.W. 3. Clouds grouped with an electric appearance as yesterday. 4. Obscurity and a little rain, a. m.: heavy showers, p. m.: rain in the night. 5. Fine, with *Cumulostratus*. 6. The same: *Cirrus* and *Cumulus* appeared: the smoke was attracted by the clouds, and a few drops of rain fell by inoculation among the latter, p. m. 7, 8. Clouds various, and mixed with haze: on the latter night a very luminous large corona round the moon. 9. Large plumose *Cirri*, followed by obscurity and *Cirrocumulus*, with an electrical character: a fine shower at evening: rain in the night. 10—15. Fair, with the lighter modifications and breezes.

## RESULTS.

Winds Easterly in the middle, Westerly in the beginning and end of the period.

Barometer: Greatest height .....	30.17 inches.
Least .....	29.40
Mean of the period .....	29.83
Thermometer: Greatest height .....	73°
Least .....	25
Mean of the period .....	51.67
Mean of the Hygrometer .....	67
Evaporation .....	2 inches
Rain .....	1.91 inch.

TOTTENHAM, *Fifth Month*, 17, 1819.

L. HOWARD.

# ANNALS

OF

## PHILOSOPHY.

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AUGUST, 1819.

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### ARTICLE I.

*Experiments to determine the Composition of the different Species of Pit-Coal.* By Thomas Thomson, M.D. F.R.S.

THE great difference which exists between the qualities of the various species of pit-coal which abound in Great Britain must have struck the most careless observer. These differences have been long known to the consumers, and have led to the preference or almost exclusive use of particular species for particular purposes. It occurred to me last winter that a more accurate determination of the constituents of our different kinds of coal than had hitherto been made was likely to throw considerable light upon their application to specific purposes, particularly the manufacture of coke and the preparation of coal gas. It is already known that some kinds of coal yield a much greater gaseous product, and of a much better quality than others. This I thought likely to depend upon specific differences in the composition of the coal, and the following experiments will show that the conjecture was well founded.

The Wernerian arrangement of black coal into six subspecies, though sufficiently minute, if not too much so, does not seem to me to be applicable to the different kinds of coal which exist in such abundance in Great Britain; at least I have never been able to arrange our British coals under it, far less to make it subservient to point out the different qualities of coal as an article of fuel. It will be necessary, therefore, in order that the following experiments be understood, to employ here a new subdivision of the various kinds of black coal. Those which I examined are the coals which occur in the neighbourhood of



Glasgow. I am far from certain that they include all the varieties of coal known in Great Britain, though, in all likelihood, at least the most important of these varieties are found in this neighbourhood, which constitutes one of the richest coal districts in the island. The species of coal which I have examined are distinguished here by the following names :

1. Caking coal.
2. Splint coal, or lightburn hard coal.
3. Cherry coal, or soft coal.
4. Cannel coal.

Besides these four species, I am aware of the existence of another, which is found in Wales under the name of *Welsh culm*, and in Ireland under the name of Kilkenny coal. This species does not burn with flame, and leaves about 95 per cent. of charcoal. Want of specimens prevented me from subjecting this species to experiment. I think it not unlikely that the coal which exists in veins in the isle of Skye constitutes likewise a different species. The want of specimens prevented me from examining it; but, as far as I recollect, it possesses many of the qualities of vegetable charcoal.

#### 1. *Caking Coal.*

The beds of coal in this neighbourhood, which are technically distinguished by the name of the Glasgow seams, are six in number. The sixth, or lowest of these beds, consists of caking coal. It is a thin bed, little more than two feet in thickness. On that account, it is nowhere wrought at present; but the same species of coal exists in many other places. Thus it is found at Bannockburn, in Stirlingshire, and in various parts of the coal district in Fife. The coal in the neighbourhood of Newcastle, which is carried to London and to all the south of England and the north of Scotland, belongs to this species. As the Newcastle variety is the most important, both on account of its quantity and the uses to which it is applied, I employed it in my experiments. It was from picked specimens of Newcastle coal that the following description was drawn up :

Colour, velvet-black; in some places (from the fracture) greyish-black.

Lustre, shining; resinous.

Principal fracture straight slaty; cross fracture partly small-grained uneven, when the lustre is only glistening; partly small conchoidal, when the lustre is shining. It is not uncommon to observe the cross fracture having exactly the appearance of wood charcoal.

It is soft, and very easily frangible. The fragments have more or less of a cubical shape.

Brittle. Soils the fingers.

Specific gravity, 1.269.

When heated, it breaks into a great number of small pieces. When the heat is raised to a certain point, it melts, and all the fragments are united together into one solid mass. This gives it the name of *caking coal*.

It catches fire very easily, and burns with a lively yellow flame; but in consequence of its caking property, it requires to be frequently stirred to admit the access of air, otherwise it is extinguished.

This species of coal is distinguished by the length of time which it burns, and the great heat which it gives out; so that when it can be procured at the same price, it is much more economical than any of the other species of coal.

## 2. *Splint Coal*.

This species of coal constitutes the fifth of the six Glasgow coal-beds, or the lowest bed at present wrought. It is thin, varying in thickness from 30 inches to three feet. It occurs also occasionally in some of the other beds, particularly the second; but does not constitute the whole of any of the beds except the fifth. It is a well characterized species, and constitutes the most valuable of the Glasgow coals, and always sells at a higher price than the cherry coal or soft coal, as it is also called. It is the only species of coal which is employed in the neighbourhood of Glasgow for the manufacture of coke. It is found to answer remarkably well for smelting iron, and is solely used for that purpose. Hence, perhaps, the reason why the iron smelted in the neighbourhood of Glasgow is considered as of a better quality, and brings a higher price in the market, than either Welsh or Staffordshire iron. This at least is the case with the iron made at the Clyde iron work, where splint coal alone is employed.

Colour black, with a slight shade of brown.

Lustre between glimmering and glistening; resinous. Lustre of the streak between glistening and shining. It is not uncommon to meet with thin layers of cherry coal running through splint coal. Such layers have a much higher lustre, and a much finer black, and, therefore, are easily distinguished from splint coal.

Principal fracture imperfect curve slaty. Cross fracture fine grained, uneven, and splintery. It is this splintery appearance that seems to have led to the name splint coal.

Not harder than either caking coal or cherry coal; but much more difficultly frangible. Hence it is commonly called *hard coal*. Fragments usually inclining to wedge-shaped.

Specific gravity, 1.290.

It requires a much higher temperature to kindle it than is necessary either for caking coal or cherry coal. It burns with flame; but lasts much longer than cherry coal. In consequence of the high temperature requisite to maintain the combustion, it



very well characterized. It does not occur in any of the six Glasgow beds of coal. What is burned in that city comes chiefly from Lismahago, and occurs in beds situated above all the Glasgow beds. It is found also in different parts of Airshire, where it is manufactured into ink-horns, snuff-boxes, and other similar trinkets. It abounds, as is well known, at Wigan, in Lancashire. There is a mine of it in Lord Anglesea's park at Beaudesert, not far from Coventry, which his lordship keeps for his own private consumption.

Colour dark greyish-black; sometimes brownish-black.

Lustre glistening; resinous. It takes a good polish.

Fracture usually large and flat conchoidal. In the great, it is frequently slaty.

In some varieties, the fragments approach the cubic shape; in others, they are wedge-shaped, or even quite irregular.

About the same degree of hardness as the other species of coal.

Brittle. Does not soil the fingers.

Much more difficultly frangible than caking coal or cherry coal; but more easily broken than splint coal.

Specific gravity 1.272.

When applied to the flame of a candle, it catches fire, and burns with a strong yellow flame without melting. This easy combustibility induces many persons to employ cannel coal to light their rooms as a substitute for candles. For this purpose, they put a piece of it on the top of their fire, and supply its place with another piece, as soon as it is consumed. If the slaty structure of the cannel coal be laid along the fire, the piece soon flies to pieces with a crackling noise, and is driven in flames to the furthest corner of the room. Hence the reason why this species is known in Scotland by the name of *parrot coal*. If the coal be laid on the fire so that its plates are perpendicular to the surface of the grate, it speedily splits into thin plates like the leaves of a book; but it does not spark out of the fire; the pieces that would otherwise fly off being intercepted by the direction of the plates.

Such is a description of the four species of coal known in the neighbourhood of Glasgow, and to which the following experiments were confined.

## II. Experiments to determine the Constituents of the four Species of Coal.

It has been hitherto the general opinion of chemists that pit-coal is a combination of bitumen and charcoal. They have endeavoured to determine the proportion of each of these constituents by subjecting the coal to distillation. The loss of weight was ascribed to bitumen, and the residual coke was considered as the proportion of the coal which consisted of charcoal. Kirwan formed the same notion respecting the



constitution of coal, and he attempted to analyze it by determining the quantity of saltpetre which a given weight of each species of coal is capable of decomposing; for this quantity he considered as proportional to the charcoal of the pit-coal; the bituminous portion being too volatile to be capable of being raised to the temperature requisite to enable it to decompose the nitre.

It seems unnecessary to make any observations upon the analyses of coal hitherto published. We have no evidence whatever that pit-coal is a compound of charcoal and bitumen. We cannot resolve it into these two constituents except by the action of heat; and these are not the only constituents obtained, when we distil pit-coal; for we always find water and ammonia in the receiver as well as bitumen. Nor are we able to prevent a considerable proportion of the coal from making its escape in the state of gas. We have, therefore, exactly the same evidence that pit-coal is composed of charcoal, bitumen, water, ammonia, carburetted hydrogen gas, and olefiant gas, as we have that it is a compound of bitumen and charcoal; but when we attempt to mix all these constituents together, we are quite unable to form any substance exactly resembling the pit-coal from which we procured them. Pit-coal seems to be nothing else than a peculiar combustible substance formed by the union of certain proportions of carbon, hydrogen, oxygen, and azote. In what way these ultimate constituents are grouped together, we have no accurate notion; but it will appear from the following experiments that every species of pit-coal is a definite compound of a determinate number of atoms of each of its constituents.

I conducted the experiments to determine the constitution of the different species of pit-coal in the following manner: 1. I ascertained the quantity of earthy matter left when each species was kept for many hours in a red heat in an open muffle. 2. I converted a given weight of each species into coke, by subjecting it to a strong heat in a close vessel, and determined the proportion of coke which each yielded. 3. I mixed a given weight of each kind of coal with peroxide of copper, and subjected the mixture to a red heat in a copper tube connected with a mercurial trough, and determined the quantity of carbonic acid and water disengaged, and the bulk of azotic gas evolved—data which enabled me to determine the quantity of carbon, hydrogen, azote, and oxygen, of which the coal was composed. I shall state in succession the results obtained in each of these three suits of experiment.

#### 1. *Earthy Matter contained in each Species.*

To determine this point, 20 gr. of each species of coal were put into a platinum crucible or cup, which was introduced into a muffle, and kept red-hot till every thing combustible was consumed and dissipated. From four to six hours are requisite to

perform this experiment aright. The muffle must be perforated with holes at its upper extremity in order to occasion a current of air through it, without which the combustion is very slow indeed. I kept 20 gr. of coal red-hot in a close muffle for eight hours; but on allowing the furnace to cool, and examining the coal, I found it by no means completely burnt. The following table exhibits the proportion of earthy matter left by 20 gr. of each species of coal tried.

	Grain.	Per cent.
1. Caking coal , , , , ,	0.3	= 1.5
2. Splint coal. . . . .	1.9	= 9.5
3. Cherry coal . . . . .	2.0	= 10.0
4. Cannel coal . . . . .	2.2	= 11.0

The cannel coal which I used in my experiments was from the Marquis of Anglesea's park, near Coventry. Its colour was brownish-black, and it was interspersed with small particles of a yellowish-brown colour, which burned like the rest of the mass.

These ashes had a white colour. They contained no lime; but were chiefly siliceous, mixed, however, with a certain proportion of alumina and oxide of iron. There is every reason for believing that this earthy matter is of the same kind with that of which the slate clay which accompanies the coal beds is composed.

#### *2. Coke yielded by each Species of Coal.*

To determine with precision the quantity of coke which a given species of coal will yield, it is obviously necessary to expose it to heat in a close vessel; for when coal is coked in the open air, a much greater loss must be sustained than what is owing merely to the volatile matter dissipated by the action of the heat. Accordingly coking ovens have been found more economical than coking in the open air. But though the iron smelters in the neighbourhood of Glasgow are aware of this circumstance, they prefer the old method of coking the splint coal in the open air. By this process they lose a portion of their coke; but they find that the quality of the iron is injured when they smelt it with coke prepared in ovens. They assign as a reason that the sulphur which the splint coal contains (under the form of pyrites) is much more completely dissipated when the coal is coked in the open air than when it is coked in ovens. Whether this explanation be satisfactory, I cannot pretend to determine; but it is considered as a settled point that the quality of the iron is improved when the coal employed to smelt it has been coked in the open air; and this improvement is considered as a full equivalent for the waste of coal that is sustained by coking it in the open air.

The truth seems to be, that as the iron makers in this neighbourhood are either great coal proprietors, or at least have leases of large tracts of coal, they have not been much in the habit of



### 3. Constituents of each Species of Coal.

The method which I employed to analyze the different species of coal was this. I reduced one grain of the coal to a fine powder, and mixed it intimately with 140 gr. of peroxide of copper in a fine powder, which had been subjected to a red heat in a crucible, and had been kept in a well-stopped glass phial. This mixture was put into a copper tube, and occupied a portion of it amounting to about four inches in length at the upper end of the tube. The copper tube was then filled with peroxide of copper. I first luted to the end of the copper tube a glass tube of the shape and size described in the *Annals of Philosophy*, xii. 109, having previously filled it with dry muriate of lime in powder. That part of the copper tube containing the mixture of coal and peroxide of copper was then heated to redness, and kept in that state till all extrication of gas was at an end. The gas was collected over mercury, its quantity was estimated, and its nature ascertained. The glass tube, by the increase of its weight, indicated the quantity of water which had been evolved during the process.

The inconveniences attending this apparatus were the following: 1. The muriate of lime was apt to melt at the upper extremity of the tube, owing partly to the water which it absorbed, and partly to the heat to which it was exposed. When this happened, it effectually prevented the gaseous product from making its way through the tube; so that the process for that time failed. 2. The increase of weight which the tube sustained varied considerably in different trials with the same coal; though the process was conducted in every respect in the same way. As the glass tube was luted to the copper tube by means of fat lute, and as this lute during the process was unavoidably raised to a temperature higher than that of boiling water, I could not be certain that some part of the increase of weight of the tube did not proceed from the lute.

To get rid of this inconveniency, I got a brass tube with a small bore (not larger than a moderate sized wire), about four inches long, bent into the shape of the upper extremity of the glass tube. One end of this brass tube was ground so as to fit exactly the extremity of the copper tube; and as brass is more expanded by heat than copper, there was no risk of its loosening during the continuance of the experiment. The other end of the brass tube was shaped so as to fit the extremity of a glass tube similar to the one formerly employed; only it was deprived of its upper end; its diameter was rather greater, and it was stouter. The end of the glass tube was cemented to the brass tube in one case (for I have several different tubes) with plaster of Paris, in another by shell lac; and I found both methods to answer equally well. The glass tube was filled with pounded muriate of lime, which was prevented from making its way into the brass



tube by means of a little amianthus with which I stuffed its extremity.

Such is the last state to which I have reduced my apparatus. But I find myself still unable to prevent considerable differences from taking place, both in the quantity of gas evolved, and of moisture absorbed by the muriate of lime, in different trials on the same coal. To what these differences are to be ascribed, it is not easy to say. I have been suspecting that the column of muriate of lime through which the gas passes (about 12 inches) is not, perhaps, long enough to deprive it effectually of all its moisture. I intend, therefore, to make trial of a column twice that length to see whether it will yield more steady results; but I suspect that the chief, if not the only cause of these differences is the difficulty of reducing the coal to a powder sufficiently fine, and of mixing it with perfect regularity through so great a proportion of peroxide of copper; for unless every particle of coal be very small, and unless it be in contact with a sufficient number of particles of peroxide of copper, it is very possible that it may escape complete combustion.

In such experiments, therefore, it would be extremely hazardous to trust to a single trial. I generally made six successive experiments upon each kind of coal, and sometimes even more. The conclusions were drawn from a mean of all the experiments, except when some obvious reason presented itself for excluding any individual experiment.

### 1. *Caking Coal.*

From one grain of caking coal, treated in the way above described, there were evolved 5.917 cubic inches of gas, supposing the barometer to stand at 30 inches, and the thermometer to stand at 60°. Of this gas, caustic potash absorbed 5.3167 cubic inches, which were considered to be carbonic acid gas. The residual gas (measuring 1.12 cubic inch) was left for 24 hours in contact with a stick of phosphorus, at a temperature which varied from 60° to 80° according to the time of the day. The 112 volumes of gas by this treatment were reduced to 98.56 volumes. The 13.44 volumes which disappeared, I considered as oxygen gas. I suppose this oxygen gas to be derived from the common air which occupies the interstices of the peroxide of copper and muriate of lime in the tubes. The amount of this common air is easily estimated when we know the volume of its oxygen; for if we multiply the volume of oxygen by five, we obtain the volume of common air. From this it is obvious that the portion of the residual gas, which was common air, amounted to 0.672 cubic inch. The remainder, amounting to 0.448 cubic inch, must have been azotic gas obviously derived from the decomposition of the grain of caking coal on which the experiment was made.

It is obvious that the place of 0.672 cubic inch of common

air, which was found in the residual gas, must have been supplied in the tubes by an equal volume of the gas extricated by the decomposition of the coal. Now this gas was a mixture of 11 parts in volume of carbonic acid gas and one part of azotic gas; so that to the volume of carbonic acid gas evolved, we must add  $\frac{1}{11}$ ths of 0.672 cubic inch = 0.616 cubic inch; and to the volume of azotic gas  $\frac{1}{11}$ th of the same quantity = 0.056 cubic inch. Hence the quantity of carbonic acid and of azotic gas evolved by the decomposition of a grain of caking coal was as follows:

	Cubic inches.
Carbonic acid gas .....	5.9327
Azotic gas .....	0.5040

During the process, the tube containing the muriate of lime had increased in weight 0.3744 grain, which is equivalent to 0.0416 grain of hydrogen.

These are the data from which the constituents of caking coal are to be estimated. They are the result of several experiments, which agreed exactly with each other in the quantity of water formed, but not in the quantity of gas evolved.

One hundred cubic inches of carbonic acid gas, when the barometer stands at 30 inches and the thermometer at 60°, contain 12.647 gr. of carbon; therefore, 5.9327 cubic inches of this gas contain 0.7503 gr. of carbon.

One hundred cubic inches of azotic gas, under the mean temperature and pressure, weigh 29.652 gr.; therefore, 0.504 cubic inch weigh 0.1494 gr.

We have, therefore, the constituents of a grain of caking coal as follows:

Carbon. ....	0.7503
Hydrogen. ....	0.0416
Azote .....	0.1494
Ashes .....	0.0150
	<hr/>
	0.9563
Loss .....	0.0437
	<hr/>
	1.0000

This loss we must consider as oxygen. Had it been any thing else, it would have made its appearance among the gaseous products.

The weight of an atom of carbon is 0.75, and the weight of an atom of hydrogen 0.125; or an atom of carbon weighs six times as much as an atom of hydrogen; but the number 7503 denoting the weight of carbon in the coal, is 18 times as great as the number 416, which denotes the weight of the hydrogen. It is obvious from this, that the coal contains three times as many atoms of carbon as it does of hydrogen.

If we compare the volume of carbonic acid gas evolved with

that of the azotic gas evolved, we shall find the former rather more than 11 times as great as the latter. From this, it follows that the number of atoms of carbon in the coal bear the ratio to that of the azote of 11 to 1.

The weight of an atom of oxygen is to that of an atom of hydrogen as 8 to 1; but the numbers 437 and 416 are to each other nearly in the ratio of equality. Hence it follows, that the number of atoms of hydrogen is eight times as great in the coal as those of the oxygen.

From these data, it is obvious that the smallest number of atoms of which caking coal can be composed is the following :

33 atoms carbon .....	=	24.750	.....	75.28
11 atoms hydrogen. . .	=	1.375	.....	4.18
3 atoms azote. ....	=	2.250	.....	15.96
1.5 atom oxygen. ....	=	1.500	.....	9.58
		<hr/>		<hr/>
		32.875		100.00

So that the weight of an integrant particle of caking coal cannot be less than 32.875; and it must be either that number, or a multiple of it.

What I consider as constituting the important part of this analysis is the establishment of the fact that caking coal contains three times as many atoms of carbon as it does of hydrogen.

## 2. Splint Coal.

This species is not so easily decomposed when heated along with peroxide of copper as the other species. The copper tube must be kept for several hours in a strong red heat. We found it necessary to urge the charcoal fire during the whole continuance of the experiment by means of bellows, otherwise the evolution of gas immediately stopped.

The mean of the trials made to determine the composition of this coal gave the following results :

From one grain of the coal, there were evolved 4.492 cubic inches of carbonic acid gas and 0.144 cubic inch of azotic gas, reduced both to the mean temperature and pressure. The water extricated during the experiment weighed 0.4 gr. equivalent to 0.044 gr. of hydrogen.

Hence the constituents of a grain of splint coal are as follows :

Carbon .....	0.568
Hydrogen .....	0.044
Azote. ....	0.043
Ashes. ....	0.100
	<hr/>
	0.755
Oxygen. ....	0.245
	<hr/>
	1.000

By a train of reasoning precisely similar to that which was followed in considering the products obtained from caking coal, it may be shown that the number of atoms of each of these constituents bear to each other the following ratios :

28	atoms carbon . . . . .	=	21.00	. . . . .	75.00
14	atoms hydrogen . . .	=	1.75	. . . . .	6.25
1	atom azote . . . . .	=	1.75	. . . . .	6.25
3 $\frac{1}{2}$	atoms oxygen . . . .	=	3.50	. . . . .	12.50
			<hr/>		<hr/>
			28.00		100.00

From the preceding table, we see that in splint coal (abstracting the ashes) there is almost as great a quantity of carbon as in caking coal; yet the latter yields more coak than the former. The reason of this apparent anomaly presents itself to our view upon comparing the atoms of carbon and hydrogen in the two species. In caking coal, the atoms of carbon are to those of hydrogen as three to one. In splint coal as two to one. It is this additional dose of hydrogen in the splint coal which enables the heat to carry off a greater proportion of the carbon than in caking coal, and of course reduces the weight of the coke. Carbon itself is not volatile when heated; it requires the presence of hydrogen to give it the gaseous state.

### 3. Cherry Coal.

This coal is decomposed with much greater facility than splint coal, when heated along with peroxide of copper. Gas comes over plentifully, though the heat to which it is raised scarcely amounts to redness.

From one grain of this coal treated in the usual way with 140 grains of peroxide of copper, I obtained the following gaseous constituents reckoned under the medium pressure and temperature.

Carbonic acid gas . . . . .	5.27 cubic inches.
Azotic gas . . . . .	0.31

The water evolved amounted to 0.9 gr. equivalent to 0.1 gr. of hydrogen.

Hence the constituents of cherry coal are as follows :

Carbon . . . . .	0.666
Hydrogen . . . . .	0.100
Azote . . . . .	0.092
Earth . . . . .	0.100
	<hr/>
	0.958
Oxygen . . . . .	0.042
	<hr/>
	1.000

These weights, when converted into atoms, become equivalent to the numbers contained in the following table :







as it does of carbon. Hence the readiness with which it burns; the great quantity of gas which it yields; and its unfitness for the purposes of coking.

The following table exhibits in one view the results of the preceding experiments on the composition of these four different species of coal.

### *I. Constituents by Weight.*

	Carbon.	Hydrogen.	Azote.	Oxygen.	Total.
Caking coal.	75.28	4.18	15.96	4.58	100
Splint coal..	75.00	6.25	6.25	12.50	100
Cherry coal.	74.45	12.40	10.22	2.93	100
Cannel coal.	64.72	21.56	13.72	0.00	100

### *II. Constituents in Atoms.*

	Carbon.	Hydrogen.	Azote.	Oxygen.	Number of atoms.
Caking coal.	33	11	3	1.5	48.5
Splint coal..	28	14	1	$3\frac{1}{2}$	$46\frac{1}{2}$
Cherry coal.	34	34	2	1	71
Cannel coal.	11	22	1	0	34

I flatter myself that these experiments, imperfect as they are, will serve very materially to guide manufacturers in the choice of their coal according to the peculiar objects which they have in view. We see from them that the goodness of a coal does not depend so much upon the absolute quantity of carbon which it contains, as upon the proportion which exists in it between the carbon and the hydrogen. When the object is to convert the coal into coke, or when we wish to make it subservient to the production of long, continued, and intense heat, we must make choice of the species which contains the greatest proportion of carbon and the smallest of hydrogen. On the other hand, when the object is to procure coal gas, we must choose the species which contains the greatest proportion of hydrogen compared to that of the carbon. For this purpose cannel coal answers best of all. It contains twice as many atoms of hydrogen as it does of carbon, and seems likewise to be destitute of oxygen, or at least nearly so. Next to the cannel coal, the cherry coal answers best for the preparation of coal gas. Caking coal is inferior to cherry coal for this purpose. I have not tried splint coal, being deterred by the great heat requisite to decompose it; but it would not in all probability yield much gas, nor of the best quality.

I have made a great variety of experiments on coal gas, but think it needless to state the results, as they contain but little

novelty. Cherry coal yields almost pure carburetted hydrogen gas. The goodness of coal gas depends very much upon the rapidity with which the heat is applied. By long continued low heats, I have never been able to obtain a gas that burned with a strong light. I have tried what kind of gas could be obtained by decomposing the coal tar; but the result did not answer my expectation. The gas obtained yielded too little light to make it worth while to prosecute such experiments further.

It is obvious, from the preceding experiments, that the four kinds of coal found in the neighbourhood of Glasgow are composed each of definite proportions of their constituents, and, therefore, that they constitute four distinct species of coal, which must be placed separately by mineralogists. I present them with these results as a first approach to the arrangement of coal. When similar experiments are sufficiently varied and extended, we shall know whether all the varieties of coal can be arranged under these four species, or whether there do not still exist other species of coal not found in this neighbourhood.

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## ARTICLE II.

### *On a Native Compound of Sulphuret of Lead and Arsenic.* By James Smithson, Esq. F.R.S.

Paris, May 19, 1819.

THIS mineral is found in Upper Valais, in Switzerland. It is lodged in a white, granose, compound carbonate of lime and magnesia. It is accompanied in this rock by regular crystals of yellow sulphuret of iron; by red sulphuret of arsenic; and by some other substances.

This compound sulphuret has a metallic aspect. It is of a grey colour; it is exceedingly brittle and soft; its fracture in some directions is perfectly vitreous; but in at least one direction, it is evidently tabular; but the size of the fragments I had, not exceeding coarse sand, precluded research with respect to crystalline construction. By trituration, this ore afforded a red powder.

At the blow-pipe, this ore melted instantly on the contact of the point of the flame. It smoked considerably; and a small flame was visible on the surface of the melted button. On cooling, this button forced out a quantity of fluid matter from its interior. During the fusion, the bead occasionally swelled up, and puffs of dense smoke issued from it; due evidently to a volatile matter, which the fire expelled from another less volatile. Finally, a button of a more fixed, less fusible, white metallic matter, extensible under the hammer, was left, and which proved to be lead.

Some bits of this compound sulphuret heated in a tube over a candle, melted, and a red sublimate rose, which became yellow on cooling, and looked like orpiment.

Some of this ore, being fused with nitre, deflagrated, and became a white oxide. The solution of this nitre afforded a white precipitate with muriate of barytes; and with nitrate of silver, a brick-red precipitate of arseniate of silver.

The white precipitate by muriate of barytes was only partially soluble in nitric acid. The insoluble part of this precipitate, of which the quantity was so minute that no balance would have been sensible to it, was carefully collected on to a very small bit of charcoal fixed to a pin. It was then strongly heated at the blow-pipe. This bit of charcoal now put into a drop of water, placed on a silver coin, immediately made a black stain of sulphuret of silver on the coin. This is the nicest test I am acquainted with of the presence of sulphur, or sulphuric acid, in bodies.

The quantity I possessed of this mineral for experiment was very small. The above trials were made with particles little more than visible; however, the results, I think, sufficiently establish the nature of the constituent parts. Their respective proportions must remain for inquiries on another scale.

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### ARTICLE III.

*Researches on a new Mineral Body found in the Sulphur extracted from Pyrites at Falun.* By J. Berzelius.

(Continued from vol. xiii. p. 412.)

*Composition of Selenic Acid.*—I have found it almost impossible to determine exactly the composition of selenic acid directly. Among the methods to which I have had recourse, none has appeared to me better than the analysis of the double acid of which I have just given an account.

To make this analysis, I took a piece of barometer tube, and blew two hollow spheres in the middle of it at the distance of two inches from each other. Into one of these balls I put a piece of selenium, after determining its weight with accuracy. I then drew out the two ends of the barometrical tube into tubes almost capillary. The object of the second ball was to receive the vapours of the double acid which might be volatilized by the heat evolved in the ball containing the selenium. The apparatus thus prepared was accurately weighed. I then introduced into it chlorine gas, which had passed through a tube 21 inches in length, and filled with dry muriate of lime. The gas was introduced by the tube till it came in contact with the selenium, and





I think I have ascertained that it is capable of combining with three times as much selenium as it contains at first. I weighed a portion of the saturated compound, and then decomposed it by water. I washed and dried the selenium separated by this liquid, and its weight exceeded a little three times that of the selenium in the portion dissolved by the water; but it was obvious that the whole of the muriatic acid had not been removed, as the filter was corroded by it during the drying. If this compound be a muriate of the oxide of selenium and not a solution of selenium in a muriate, the oxide of selenium which it contains should be a compound of two atoms of radical and one of oxygen. This is the case likewise with the oxide of sulphur supposed to exist in the muriate of the oxide of sulphur (the protochloride of sulphur of the new hypothesis respecting the nature of muriatic acid).

*Reduction of Selenium to the State of a Combustible.*—Selenic acid is easily reduced both by the moist and the dry way. A solution of selenic acid mixed with muriatic acid produces no trace of chlorine, even when heated. If into this solution we introduce a piece of zinc or of polished iron, the metal immediately assumes the colour of copper, and the selenium is gradually precipitated in the form of red, or brown, or blackish flocks, according as the temperature is more or less elevated. If we mix selenic acid with sulphuric acid, the selenium precipitates more slowly, and contains sulphur. If the selenic acid contains arsenic acid, or seleniate of mercury, the selenium is still more difficult to be precipitated by zinc or iron.

The most convenient mode of separating selenium from a solution is to render the liquid slightly acid, and then to pour into it sulphite of ammonia. The uncombined acid disengages the sulphurous acid, by the action of which the selenic acid is gradually decomposed. The liquid, at first limpid, assumes a yellow colour, becomes muddy, acquires a fine red colour, and after an interval of some hours deposits red flocks in abundance. The selenium is not completely precipitated cold, the liquid must be boiled for half an hour, during which fresh sulphite of ammonia must be occasionally added. The precipitate formed by boiling is dark grey, or almost black. If the liquid contain nitric acid, the selenium is not completely precipitated till after the whole nitric acid is decomposed by the sulphurous acid. In such a case, it is better to add muriatic acid to the liquid, and to evaporate till the nitric acid be decomposed, to redissolve in water, and then add sulphite of ammonia.

I have already mentioned that when seleniate of potash is heated with muriate of ammonia, selenium is obtained by the deoxidizing property of the ammonia; but in this case we always lose a small quantity of selenium which comes over with the water in the form of an acid; and if the heat be not continued long enough, the upper layer in the retort may contain



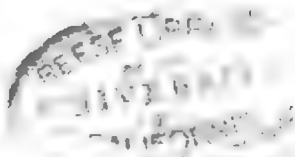
least at first, are hydroseleniurets, which, by the influence of atmospherical air, are converted in a short time into seleniurets of the oxide, as I shall prove below.

Seleniuretted hydrogen gas is easily decomposed by the action of water and air. If the gas comes in contact with a moist body, humidity absorbs it, the oxygen of the air decomposes it, and the humid body assumes a cinnabar red colour. The selenium thus deposited penetrates porous bodies, especially organic bodies, so as not to be separated from them mechanically. A piece of moist paper becomes red in the interior; a piece of wood is penetrated to some distance below the surface; and a tube of gum elastic, which had been employed in an experiment to conduct the seleniuretted hydrogen gas, had its interior coloured entirely of a fine red.

This gas produces effects on the trachea and on the organs of respiration, which may probably become dangerous. On the interior of the nose, its effect is to produce at first an odour exactly resembling that of sulphuretted hydrogen gas; but immediately after a sharp, astringent, painful sensation is perceived on all those parts which the gas has touched. This sensation is very analogous with that which is caused by silicofluoric gas; but this last gas produces but little effect when compared to that of seleniuretted hydrogen. The eyes become immediately red and inflamed, and the sense of smelling is quite lost.

In the first experiment which I made on the odour of this gas, I conceive that I let up into my nostrils a bubble of gas about the size of a small pea. It deprived me so completely of the sense of smell that I could apply a bottle of concentrated ammonia to the nose without perceiving any odour. After five or six hours I began to recover the sense of smell; but a strong rheum continued for about 15 days.

On another occasion, while preparing this gas, I perceived a slight hepatic odour, because the vessel was not quite close; but the aperture was very small; and when I covered it with a drop of water, small bubbles were seen to issue about the size of the head of a pin. I put the apparatus under the chimney of the laboratory, not to be incommoded with the gas. I felt at first a sharp sensation in the nose, the eyes became red, and symptoms of rheum began to appear; but only to an insignificant extent. In half an hour, I was seized with a dry, painful cough, which continued for a long time, and which was at last accompanied by an expectoration, whose taste was entirely analogous to that of the vapours of a boiling solution of corrosive sublimate. These symptoms were removed by the application of a blister to the breast. The quantity of seleniuretted hydrogen gas, which on each of these occasions acted on the organs of respiration, was much smaller than would have been required of any other inorganic substance whatever to produce sensible effects. It is







very easily with 100 parts of liquid sulphur, and the sulphur acquires a dirty-yellow colour. A little sulphur being added to the sulphuret of selenium (of which we shall speak immediately) does not diminish its transparency; but renders its colour paler. A greater quantity of sulphur destroys the transparency.

The only method of obtaining a sulphuret of selenium of a determinate composition is to precipitate a solution of selenic acid by sulphuretted hydrogen gas. The liquid becomes muddy, and assumes a fine lemon colour; but the sulphuret of selenium is not easily separated from the liquid. If we add a few drops of muriatic acid, it separates more easily; and when we heat the liquid, the precipitate coheres together, forming an elastic mass of a deep orange colour. In this combination, 100 parts of selenium are united to 60½ parts of sulphur. It contains an atom of selenium and two atoms of sulphur. The sulphuret of selenium thus obtained is very fusible. At the temperature of boiling water it becomes soft, and the different pieces of it unite into one. When heated a few degrees higher, it becomes fluid. At a still higher temperature, it boils, and may be distilled over. The portion thus distilled, when cold, is transparent, of a red orange colour, and resembles melted orpiment.

Sulphuret of selenium is attacked with difficulty by nitric acid. Nitromuriatic acid dissolves it with much greater facility. There remains a sulphur of an impure yellow, and sometimes spotted-red, which retains a portion of the selenium with much obstinacy. At last, the sulphur liquifies in the boiling liquid; and when it becomes yellow on cooling, it contains no more selenium.

Sulphuret of selenium is soluble in the fixed caustic alkalies, as well as in the alkaline hydrosulphurets. The solution has a very deep orange colour. The acids precipitate from it sulphuret of selenium.

If we heat this sulphuret in the air till the sulphur catches fire, we perceive at first the odour of sulphurous acid, which by degrees becomes mixed with that of horseradish, and at last this odour prevails. If the air has not free access, selenium likewise sublimes. If we heat together selenic acid and sulphuret of selenium, the acid oxidizes the sulphur; and I conceive that by employing the requisite proportion of each, we may, by this method, deprive selenium entirely of sulphur.

*Phosphuret of Selenium.*—When we let a piece of selenium fall on melted phosphorus, the selenium is speedily dissolved, and the solution of it sinks in the melted phosphorus in reddish striæ, which gradually mix with the fused mass, so that selenium may be mixed with phosphorus in all proportions. If we saturate phosphorus with selenium, we obtain a very fusible compound, which, when cold, has a dark-brown colour, much lustre, and a vitreous fracture.

If we digest phosphuret of selenium in water, a small portion

of the phosphorus decomposes the water, which becomes impregnated with seleniuretted hydrogen gas, acquires a hepatic odour, becomes muddy, and deposits selenium when left in contact with the air. If we boil the phosphorus in a caustic potash ley, it dissolves, and the liquid contains phosphate with hydroseleniuret of potash, and when left exposed to the air lets fall selenium, just as if it were a pure hydroseleniuret of potash.

*Seleniuret of Carbon.*—I have not examined whether selenium and carbon are capable of combining; but some phenomena which take place when the alkaline seleniates are decomposed by charcoal at a high temperature lead me to suspect that such a combination may exist. We may presume likewise that it ought to be analogous to the sulphuret of carbon, and that, like that substance, it is capable of combining with the saline bases.

#### 7. *Metallic Seleniurets.*

Selenium combines with the metals, and on that occasion performs the part of an electronegative substance. With the greater number of them, it produces the phenomena of fire, as sulphur does, but with less intensity.

The reason why it does not produce fire with all of them is the same as for sulphur; namely, that several of the metals which ought theoretically to produce the phenomena of fire with the greatest intensity require a much higher temperature to produce the combination than the boiling point of selenium. The consequence is, that the selenium evaporates before the mixture acquires a sufficient degree of heat to produce the combination with rapidity. This is the case with iron and zinc.

The seleniurets have mostly the same external characters as the corresponding sulphurets. They have in general a metallic aspect; they are, with a few exceptions, more fusible than the metals which they contain; and when they are heated to redness in the air, the selenium burns slowly with a blue flame, giving out the odour of horseradish. It is much more difficult to separate the selenium by roasting than the sulphur, because selenium has a weaker affinity for oxygen. The metallic seleniurets are soluble in nitric acid, though less easily than the same metals without selenium. Some, however, as seleniuret of mercury, are scarcely attacked by nitric acid.

Selenium evidently combines with the metals in definite proportions. In this respect, selenium corresponds with sulphur. Thus copper is capable of uniting with two proportions of selenium, one of which is obtained, when a salt containing peroxide of copper is precipitated by seleniuretted hydrogen gas. On distilling this precipitate, one half of the selenium is volatilized, and there remains in the retort a protoseleniuret of copper. This compound occurs in nature as a mineral, as I shall have occasion to show hereafter. It is well known that this is exactly the case with the compounds of sulphur and copper. The best way of

obtaining metallic seleniurets with fixed proportions is doubtless to precipitate metallic solutions with seleniuretted hydrogen gas; but we may likewise obtain the protoseleniurets by heating the metals with an excess of selenium, and drawing off this excess by distillation.

1. *Seleniuret of Potassium.*—Selenium, when heated with potassium, combines with it, and occasions a red heat, which sublimes a small quantity of the compound. Seleniuret of potassium constitutes a metallic button, of the colour of iron, which separates easily from the glass, and which has a crystallized and radiated fracture. It dissolves in water without the disengagement of gas. The solution has a deep red colour, similar to that of strong beer. The acids precipitate selenium from it, because the hydroseleniuret of potash formed by the solution has the property of dissolving the excess of selenium present, and of forming with it a seleniuretted hydroseleniuret of potash.

If we mix selenium with an excess of potassium, the combination takes place with a strong explosion, and the mass is thrown out of the vessel by excess of potassium, which is converted into vapour by the heat. The compound, when thrown into water, is dissolved with the disengagement of hydrogen gas; but notwithstanding this circumstance, the liquid assumes a red colour.

2. *Seleniuret of Zinc.*—It is equally difficult to combine this metal with selenium as it is with sulphur. If we heat together zinc and selenium in vessels in which the air cannot be received, the selenium melts, and spreads itself on the surface of the zinc, which becomes, as it were, amalgamated with it. By increasing the heat, we cause the selenium to volatilize, leaving the zinc covered with a yellow-coloured pellicle. If, on the other hand, we bring zinc heated to redness in contact with the vapour of selenium, the mass takes fire, explodes, and the inner surface of the vessel is covered with a pulverulent substance of a lemon colour, which retains its colour when cold. This powder is seleniuret of zinc, very analogous to sulphuret of zinc, which, when produced in the same way, is a yellowish-grey, non-metallic powder. We easily see that the yellow powder is not an oxide of zinc, as nitric acid dissolves it with the evolution of nitrous gas. In this case, the powder becomes first red by the solution of the zinc, and the separation of the selenium, which afterwards dissolves in the acid.

3. *Seleniuret of Iron.*—If we mix iron filings and selenium in powder, and heat the mixture to redness, the iron combines on the surface with selenium, but without any combustion; but if we put selenium at the bottom of a glass tube, introduce iron filings above it, and then heat strongly, the selenium is volatilized, and its vapours, passing through the hot iron, combine with it, producing an ignition which continues as long as any selenium is





ARTICLE IV.

*A Description of an improved Microscope for opaque Objects.*  
By Mr. Hall. (With a Plate.)

(To Dr. Thomson.)

SIR,

39, Southampton-row, March 31, 1819.

HAVING spent much of my time lately in microscopical pursuits, I have found the usual methods of exhibiting opaque objects very defective, in consequence of the want of light and the complexity of the instrument, both of which evils I have taken great pains to remedy; and, I trust, the following improvement will be found generally useful, and contribute much to the amusement of those who delight in studying the works of nature. I, therefore, enclose a drawing and description of the improved apparatus.

I am, Sir, your most obedient servant,

RICHARD HALL.

A (Plate XCV) represents the instrument put together ready for use; it is of a tubular form  $3\frac{1}{4}$  inches in length, and three in diameter, consisting of two lenses at the end \* one double convex, the other, plane convex, their combined focus being equal to the length of the tube.

B represents a sliding door, with an aperture in its centre, moving backwards and forwards by means of a pinion.

C, a moveable cup, with a scioptic ball and a pair of forceps through its centre, for the purpose of holding and adjusting the object.

D, a small hole to observe whether the object be in its right place.

E, a circular piece of brass that screws to the tube, A, having a hole in its centre to receive the nut, F, which is also a circular piece of brass, of  $1\frac{1}{4}$  inch in diameter, having three screws; namely, one male, *q q*, to screw to E; two female screws, one, *z z*, for the speculum, the other, *k k*, for the compound body.

Having thus described the several parts of the instrument, I have only to say a few words with respect to the method of using it. When put together, as at A, it should be screwed to a lantern containing an Argand lamp; then the cap, C, is to be taken off, and the object placed between the forceps; upon replacing the cap, C, looking through the hole, D, for the purpose of seeing that the light be thrown upon the object, the instrument is then ready for use, and is adjusted to its proper focus by turning the pinion  $\phi$ .

When a deeper speculum is required, it is necessary to unscrew E for that purpose. The advantages that this instrument possesses over those generally in use may be comprised in a few words; viz. it is most simple in its construction, it

requires little trouble in using, it exhibits opaque objects in a manner superior to any instrument which I have ever yet inspected, and may be constructed at a considerably less expense.

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## ARTICLE V.

*Memoir on the Quantity of Rain, and the Number of Days of Rain, Snow, and Drizzle, at Viviers, during 40 Years (Lat.  $44^{\circ} 29' 1''$  N. Long.  $2^{\circ} 20' 55''$  E. from the Meridian of Paris, 30 Toises above the Level of the Sea). By M. Flaugergues.\**

THE rain gauge which I employ for measuring the rain water that falls consists of a square vessel of tin plate covered with oil paint, the opening of which, constituting the mouth, and the bottom, are each six inches square. This vessel is placed in the town in the middle of a spacious court, on a small stone column raised six feet nine inches above the pavement, and not screened from the sky in any direction. As soon as the rain has ceased, I measure the water which has fallen into the gauge by pouring it into a glass cylinder accurately divided into half cubic inches in the following manner: I poured successively into the cylinder placed horizontally equal quantities of rain water of the temperature  $10^{\circ}$  ( $50^{\circ}$  Fahr.), weighing each 187.25 gr. (153.61 gr. troy), which is the weight of half a cubic inch at that temperature.† After every addition, I marked upon a slip of paper, pasted to the cylinder, the height of the water. I then numbered these lines, and covered the paper with varnish, that it might not be removed from the glass by humidity. The interval corresponding to a cubic inch being more than two lines, it was easy to estimate the eighth part of that quantity, which corresponds to  $\frac{1}{8}$  line of height in the rain gauge. It is evident, since the surface of the rain gauge is 36 inches, that 36 cubic inches of rain collected in it are equivalent to a depth of rain of one inch, and three inches collected in the gauge are equivalent to one line of rain in depth. It is according to this ratio that I have reduced to inches and lines of height the quantities of rain water fallen into the gauge and measured in cubic inches.

With respect to snow, which is merely congealed water, I measured the water which it produced, when melted, in a close vessel at a moderate heat.‡ I took the same method with hail.

\* Translated from the *Bibliothèque Universelle*, viii. 127. The difference between the meridian of Paris and of Greenwich is  $9^{\circ} 20'$  in time. A French toise = 76.7344 English inches.

† The French half cubic inch is equal 0.60525 English inches; the weight of which at  $50^{\circ}$  is a very small fraction of a grain less than stated in the text.—T.

‡ When the depth of the snow surpassed six inches nine lines (the height of my

Meteorologists do not appear to me to have taken into their account the water which remains attached to the sides of the gauge after they have emptied it. To determine its amount, I weighed the instrument while dry, I then moistened it in the inside with rain water, and after allowing the water to run out, I weighed it again. The weight was increased by 87 gr. (the mean of several trials). Now  $374.5 : 87 :: 1 \text{ cubic inch} : 0.2323 \text{ cubic inch}$ . This last is the amount of the rain retained by the gauge, which corresponds to 0.0741 line in height, or nearly  $\frac{1}{17}$ th of a line. I always added this last quantity to all the measures of rain. This water, which remains attached to the gauge, ought not to be neglected, since in a whole year, supposing 98 days of rain, it amounts to more than 7.25 lines. The whole height of the rain water which has fallen at Viviers in the course of 40 years; namely, between 1777 and 1818, amounts to 113 feet 3 inches and 4 lines. This quantity, divided by 40, gives 33 inches 11.8 lines, for the mean height of the rain which fell each year; and this quantity is divided among the months of the year in the following manner:

	Inches.	Lines.		Inches.	Lines.
January . . . . .	2	5.73	July . . . . .	1	10.59
February . . . . .	1	8.46	August . . . . .	2	4.17
March . . . . .	1	11.13	September . . . . .	4	1.68
April . . . . .	2	8.23	October . . . . .	4	8.89
May . . . . .	2	11.17	November . . . . .	4	2.24
June . . . . .	2	6.75	December . . . . .	2	4.76

If we took the mean annual quantity of rain for unity, we shall find the mean monthly quantity as follows:

Winter 0.1798	{	January . . . . .	0.0729
		February . . . . .	0.0502
		March . . . . .	0.0567
Spring 0.2407	{	April . . . . .	0.0790
		May . . . . .	0.0860
		June . . . . .	0.0757
			<hr/>
			0.4205
Summer 0.2462	{	July . . . . .	0.0553
		August . . . . .	0.0691
		September . . . . .	0.1218
Winter 0.3333	{	October . . . . .	0.1395
		November . . . . .	0.1232
		December . . . . .	0.0706
			<hr/>
			0.5795

rain gauge) I detached a prism of snow, the base of which was a square of six inches the side, from the snow which had fallen in the court, and measured the water which it yielded when melted.



The quantity of rain which falls in autumn and winter is to the quantity which falls in spring and summer nearly as 13 to 12; and the quantity of rain in winter and spring is to the rain in summer and autumn nearly as 7 to 10.

But this table does not give the exact rate of the rain in each month, because the months have not all the same number of days; and on that account, the longest ones ought to exhibit a greater quantity of rain; and the shortest, a smaller quantity, than that which results from their constitution. We must, therefore, reduce the quantity of rain given for each month in the preceding table to what it would be if each month were equal to the 12th part of the mean year (365 days 6 hours); that is to say, equal each to 30 days 16·5 hours. When this reduction is made, the preceding table will be changed into the following one:

January .....	0·0716	July .....	0·0544
February .....	0·0541	August .....	0·0679
March .....	0·0557	September .....	0·1236
April .....	0·0802	October .....	0·1370
May .....	0·0847	November .....	0·1250
June .....	0·0765	December .....	0·0693

From this table, it appears that the months, when arranged according to their quantities of rain, beginning with the most rainy, assume the following order:

October, November, September, May, April, June, January, December, August, March, July, February.

The month of May comes nearest to the mean quantity of rain, which is 0·0833. Four months are above this quantity, and may, therefore, be considered as rainy. Eight months are below it, or dry.

That the order of the rain in a mean year may be the better understood, I have expressed it graphically in fig. F (Pl. XCV). For this purpose, I took upon the line, M N, the axis of the abscisses, 12 equal intervals to represent the 12 months of the year. From the middle of each of these intervals, I have raised a perpendicular ordinate, of a length proportional to the numbers in the above table, beginning with the month of February, and through the extremities of these ordinates I have passed a curve line, A B C D E. This curve is very regular, with the exception of the ordinate, corresponding to the month of January, which is a little too long relatively to the ordinate which corresponds to the month of December; an irregularity occasioned by the excessive rains which took place in January, 1814; but it would doubtless disappear if the observations were continued for some years longer. This curve has two *minima* and two *maxima*. The first minimum corresponds to the month of February, the second to the month of July. The first maximum corresponds to the month of May, and the second to the month of October. The two *minima* are nearly equal; but the maximum of October

is much greater than that of May. We see from this that the minima correspond nearly to the coldest and hottest seasons of the year, and the maxima to the mean temperature of the year.\*

During the course of these 40 years, the most rainy has been 1801; in which there fell 48 inches and 1 line of water; and there were 141 rainy days. The driest was 1779, in which there fell only 20 inches  $7\frac{1}{2}$  lines of water, and in which there were only 69 rainy days.

If we add the quantities of rain which fell during every decade of these 40 years taken separately, and divide the sum by 10, to have the annual mean for each of these four decades of years, we shall obtain the following quantities :

Decades of years.	Quantity of rain fallen during each decade.			Mean annual quantity of rain.	
	Feet.	In.	Lines.	In.	Lines.
1778—1787 .....	25	11	2	31	$1\frac{1}{2}$
1788—1797 .....	27	8	$2\frac{1}{2}$	33	$2\frac{3}{8}$
1798—1807 .....	28	5	$11\frac{1}{2}$	34	$2\frac{1}{2}$
1808—1817 .....	31	1	$11\frac{1}{2}$	37	$4\frac{3}{8}$

We see by this table that there is a sensible increase in the mean annual quantity of rain as we proceed from 1778, the time when these observations began, and especially during the last decade. This remark does not accord with the common notion that countries covered with wood are the most rainy; for since the beginning of these observations, and especially during the last 10 years, the forests have been very much destroyed, not only on the territory of Viviers, but over the whole department of Ardeche; and at present there remain only a few inconsiderable portions of soil covered with wood.

The heaviest rain which I have observed is that of Sept. 6, 1801. It rained during 18 hours without interruption, and there fell 13 inches  $2\frac{1}{2}$  lines of water.

During these 40 years, making altogether 14609 days, there have been 3921 rainy days, which, in round numbers, amounts to 98 rainy days in the year. These 3921 rainy days were divided among the four decades as follows :

Decades.	Rainy days.	Decades.	Rainy days.
1778—1787 .....	830	1798—1807 .....	1062
1788—1797 .....	947	1808—1817 .....	1082

And divided among the 12 months of the year as follows :

\* On comparing the observations of the quantity of rain made at different places, I think I perceive that the second maximum is greatest in countries situated near the Mediterranean, and the first maximum greatest in the countries situated near the ocean.



		Days.
Winter 941	January .....	345
	February. ....	281
	March .....	315
Spring 1001	April .....	363
	May. ....	335
	June .....	303
		<hr/> 1942
Summer 726	July. ....	229
	August .....	198
	September .....	299
Autumn 1253	October .....	419
	November .....	433
	December .....	401
		<hr/> 1979

We see by these tables that the number of rainy days in the year at Viviers increases sensibly. That the order of the months relative to the number of rainy days is this : November, October, December, April, January, May, March, June, September, February, July, August ; that the sum of the rainy days of winter and spring is nearly equal to that of the rainy days of summer and autumn ; and that the number of rainy days during autumn and winter is to the number of rainy days during summer and spring as nine to seven.

I have traced from the numbers in the preceding table (following the same method as for the curve of the mean monthly quantity of rain), a curve, which may be called the curve of the frequency of rain, the ordinates of which are proportional to the number of rainy days in each month. This curve, F G H I K (fig. F), has two minima and two maxima. The first minimum corresponds to the month of February, the first maximum to the month of April ; the second minimum to the month of August, and the second maximum to the month of December.

I have traced likewise the same curve for all the days of the year, taking ordinates proportional to the number of times that it has rained on the same day of the year during the 40 years' observations. This curve presents singular irregularities : very dry days placed in the middle of a group of wet days ; and very wet days in the midst of a series of dry days, &c.

During the course of these 40 years, the days of the year on which it has rained the oftenest are Oct. 31, on which it rained 23 times, and Nov. 4, on which it rained 21 times ; and the days on which it rained the least are July 7 and 13, on which it rained only twice.

If we divide the total quantity of rain water which has fallen

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at Viviers during 40 years; namely, 16312 lines by 3921, the number of rainy days during that space of time, we obtain 4.160 lines for the mean quantity of rain that falls in a day. And if we divide the quantities of rain that fell during each decade by the number of rainy days in that decade, we shall obtain the mean quantity of rain which fell during a day during each decade as in the following table:

Decades of years.	Intensity of diurnal rain. Lines.	Decades of years.	Intensity of diurnal rain. Lines.
1778—1787 . . . . .	4.499	1798—1807 . . . . .	3.864
1788—1797 . . . . .	4.210	1808—1817 . . . . .	4.148

From this, we may conclude that the intensity of the diurnal rain is subject to vary; for having diminished during the first three decades, it has augmented in the last.

Finally, if we divide the quantities of rain which fell each month by the number of rainy days corresponding to that month, we shall obtain the mean quantity of diurnal rain during each month as in the following table:

	Lines.		Lines.
January . . . . .	3.447	July . . . . .	3.946
February . . . . .	2.912	August . . . . .	5.690
March . . . . .	2.938	September . . . . .	6.647
April . . . . .	3.551	October . . . . .	5.431
May . . . . .	4.199	November . . . . .	4.641
June . . . . .	4.059	December . . . . .	2.869

This table gives the ratio of the intensity of the rain. We see that it is greatest in the month of September; or during that month the greatest quantity of rain falls in the shortest time. Hence it is during this month, or in October, that the great inundations of the torrent of Escoutay take place. This torrent flows into the Rhone, about 250 toises to the north of Viviers. The most violent of its inundations happened on Sept. 2, 1703, when its waters reached the very gates of the city. The lightest rains are those of the month of December.

After a long series of meteorological observations, I find that at Viviers the mean number of serene days in the year is 123; of cloudy days, 173; of days absolutely overcast, 69; of foggy days, 52; of snowy days, 5; of hail, 3; and of thunder, 24.

Lord Charles Cavendish and Dr. Heberden had remarked (Phil. Trans. vol. lxi) that the quantity of rain which falls upon the same surface at the top of a building is less than at the bottom of it. This fact has just been verified at the Paris observatory with two equal vessels placed, the one on the parapet, and the other in the court. The rain collected in the latter vessel surpassed considerably what was collected in the former.

To find the cause of this difference, it is sufficient to consider that the air is seldom perfectly calm during rain. A wind of more or less violence usually blows, which, pushing horizontally the drops of rain in their vertical fall, gives them a direction inclined to the horizon. Hence it follows that less rain will fall into the horizontal opening of the rain gauge when the rain is inclined than if it fell vertically or in a direction less inclined.

Let us suppose that the parallel and inclined lines (fig. G) represent the direction in which the drops of rain move in a vertical plane, and that the line A B is a section of the rain gauge in the same place. Let us draw A D perpendicular to the direction of the rain, and equal to A B; it is evident that all the rain which enters into the rain gauge by A B, in consequence of its inclined direction, may be intercepted by the part A C of the line A D which receives it perpendicularly; but if the rain fell vertically into the rain-gauge, the part A B would receive as much as the line A D receives in the present case; therefore, the quantity of rain which enters into the rain gauge when its direction is inclined is to the quantity which would enter into the same vessel if the rain fell vertically as A C to A D (= A B), or in consequence of the right angled triangle, A C B, as the sine of the angle, A B C, to radius; that is to say, that the quantity of rain which enters into the rain gauge is proportional to the sine of the angle of inclination of the rain.\*

The rain at the top of the house, or any other part completely exposed, experiencing without obstruction the whole action of the wind, must assume a direction inclined to the horizon, and reach the rain gauge placed there in that direction; but if the rain is screened from the action of the wind by the building, the drops gradually losing their inclined direction by the resistance of the air must fall into the rain gauge placed there vertically, or nearly so. Hence more rain must enter a rain gauge placed at the bottom of a building than into one placed on the parapet, as has been observed by the acute philosophers above named.

This remark may give origin to the question, where the rain gauge for meteorological purposes should be placed. I answer, that the solution of the question depends upon the object in view. If we wish to know the quantity of rain which falls upon a

\* I employed myself formerly in measuring the inclination of rain, at least the greatest inclination which took place during a storm, &c. The *clisometer*, which I contrived for this purpose, consisted in a very thin circular plate of metal, kept in a situation exactly horizontal by a long cylindrical support of wood, the axis of which passed through the centre of the plate, and which was fixed vertically and solidly on a small stone pedestal in a place quite exposed. The wooden support was painted red, which became much deeper when moistened. After the rain, I measured the length of the part of the wooden support which remained dry, and I calculated this proportion: the difference of the semidimeters of the metallic plate and the wooden support is to the length of the part of the support which remained dry as radius is to the tangent of the angle of inclination of the rain. Further, by the circular extent and the disposition of the moistened part along the support, I could discover the winds which had blown during the storm.

determinate space of ground, we must place the rain guage in a situation completely exposed; but if we wish to examine the phenomena of rain, and determine its intensity, as the inclination of the rain in consequence of the wind is accidental, we must place the rain guage so that its mouth shall be perpendicular to the direction of the rain; but as it is not possible to place it so, the inclination of the rain varying continually, we must contrive matters so that the rain shall fall into the guage in as perpendicular a direction as possible. For this purpose, it is convenient to place the guage in a spacious place surrounded with high buildings, which, preventing the rain from being acted on by the wind, allow it to assume nearly its primitive vertical direction. It appears to me that it is very proper to employ at once these two methods, which are capable of furnishing interesting comparisons. I have, therefore, placed near my observatory in a place quite open, a rain guage perfectly equal, and similar to that which is placed in the court of my house, and I have begun to register the quantity of rain which is collected by each of these instruments.

*Annual Quantity of Rain and rainy Days at Viviers during 40 Years.*

Years.	Quantity of rain.		Number of rainy days.	Years.	Quantity of rain.		Number of rainy days.
	Inches.	Lines.			Inches.	Lines.	
1778.	29	11 $\frac{1}{2}$	86	1798.	34	3	104
1779.	20	7 $\frac{1}{2}$	69	1799.	28	5 $\frac{1}{2}$	109
1780.	33	5	74	1800.	46	2 $\frac{1}{2}$	110
1781.	23	6 $\frac{1}{2}$	71	1801.	48	1	141
1782.	28	5 $\frac{1}{2}$	78	1802.	30	4 $\frac{1}{2}$	94
1783.	34	2 $\frac{1}{2}$	96	1803.	25	5 $\frac{1}{2}$	101
1784.	39	2 $\frac{1}{2}$	83	1804.	38	8	109
1785.	33	11 $\frac{1}{2}$	74	1805.	22	3 $\frac{1}{2}$	104
1786.	33	8 $\frac{1}{2}$	114	1806.	40	7 $\frac{1}{2}$	118
1787.	34	0 $\frac{1}{2}$	85	1807.	31	6 $\frac{1}{2}$	72
1788.	35	7	90	1808.	43	3 $\frac{1}{2}$	110
1789.	39	0	98	1809.	38	3	112
1790.	37	0 $\frac{1}{2}$	96	1810.	44	5 $\frac{1}{2}$	109
1791.	36	6 $\frac{1}{2}$	95	1811.	37	5 $\frac{1}{2}$	106
1792.	42	6	95	1812.	36	1	106
1793.	33	11 $\frac{1}{2}$	76	1813.	38	10 $\frac{1}{2}$	104
1794.	30	3 $\frac{1}{2}$	88	1814.	38	7 $\frac{1}{2}$	110
1795.	21	0 $\frac{1}{2}$	81	1815.	30	5 $\frac{1}{2}$	100
1796.	30	6 $\frac{1}{2}$	112	1816.	37	10	122
1797.	25	8 $\frac{1}{2}$	116	1817.	28	7 $\frac{1}{2}$	103





appears, that both  $M - m$  and  $P - p$  vary in a duplicate ratio of the sine of latitude nearly,  $3 e m$  and  $p e$  being given.

6. At the poles  $s = 1$  and  $c = 0$ ; then from the above expressions, we deduce  $M - m = 3 e m$ ,  $P - p = p e$ ,  $P = M$ ,  $m = p$   $\left(\frac{1+e}{1+3e}\right) = p(1-2e)$ ,  $p = m\left(\frac{1+3e}{1+e}\right) = m(1+2e)$  and  $L = 0$ .

7. An approximate value of  $e$  may be found by the following proportion; viz.  $1 + 3 e s^2 - 2 e : 1 + 3 e S^2 - 2 e :: 1 + 3 e s^2 : 1 + 3 e S^2 :: M : M'$  (Art. 1 and 3) from whence  $e = \frac{M - M'}{3(M S^2 - M' s^2)}$  nearly; where  $M'$  and  $M$  represent the lengths of a meridional degree in two given latitudes, the sines of which being  $S$  and  $s$  respectively.

8. Or an approximate value of  $e$  may be found as follows; by Art. 3 we have  $1 - 2 e + 3 e s^2 : 1 + e s^2 :: M : P$ , or by substituting  $1 - c^2$  for  $s^2$ , it will be  $1 - 3 e c^2 + e : 1 - e c^2 + e :: M : P$ , or  $1 - 3 e c^2 : 1 - e c^2 : M : P$ , from whence  $e =$

$\frac{P - M}{(3 P - M) \cos^2 \text{ lat.}}$  nearly,  $M$  and  $P$  being the lengths of a meridional and perpendicular degree in any given latitude.

By omitting  $e$  in the above proportion, the denominator is evidently too great, but may be reduced by writing  $3 M$  for  $3 P$  ( $P$  being greater than  $M$ , p. 150 "Elements of the Ellipse") we shall then have  $e = \frac{P - M}{2 M \cos^2 \text{ lat.}}$  nearer than before, but not so correct as given in Art. 7.

9. By having the measure of a meridional degree at the equator and the compression, to find the equatorial diameter and polar axis. Put  $CH$  for half the equatorial diameter, and  $CK$  for half the polar axis, then (Art. 3)  $\frac{CK^2}{CH} = \frac{CK}{CH} \times CK = \frac{1-e}{1} \times CK = v m \therefore CK = \frac{v m}{1-e}$ ; but by Art. 1,  $1 - e : 1 :: CK : CH$ ; or  $1 - e : 1 :: \frac{v m}{1-e} : CH = \frac{v m}{(1-e)^2}$ .

10.  $\frac{CH^2}{CK} = \frac{CH}{CK} \times CH = \frac{1}{1-e} \times \frac{v m}{(1-e)^2} = \frac{m v p}{m} \times \frac{1}{1-e} = \frac{v p}{1-e}$  (Art. 4 and 9) equal to the radius of curvature at the poles (Art. 3). Now if  $M$  represent the measure of a degree at the poles, then we shall have  $\frac{v p}{1-e} = v M$  ther.  $M = \frac{p}{1-e}$ , and  $e = 1 - \frac{p}{M}$ . By comparing the value of  $e$  in Art. 4 with the above, we have  $\sqrt{\frac{m}{p}} = \frac{p}{M} \therefore M = p \sqrt{\frac{p}{m}}$ .

11. The quadrantal arc of the elliptic meridian is equal to



degrees perpendicular to the meridian, then will  $M' = 3 (P' - P) + M$  and  $P' = \frac{M' - M}{3} + P$  very near.

Let  $S', S$  denote the corresponding latitudes,  $m$  the length of a meridional degree,  $p$  the length of a perpendicular degree at the equator, and  $e$  the compression; then (Art. 5)  $M' - M = 3 e m (S'^2 - S^2)$  and  $P' - P = p e (S'^2 - S^2)$ , therefore  $M' - M : P' - P :: 3 m : p :: 3 : 1$  nearly, or  $M' - M = 3 (P' - P)$ , hence we have  $M' = 3 (P' - P) + M$ , and  $P' = \frac{M' - M}{3} + P$  very near.

17. The preceding equations are nearly the same as given by Col. Lambton, in Part II. of the Philosophical Transactions, for the year 1818, from whence most of the following examples are taken.

*Example 1.*—Given 60473.53 fathoms for the length of a meridional degree in latitude  $9^\circ$  N. and 60484.5 fathoms for the length of a meridional degree in latitude  $12^\circ$  N. to find the compression.

$$\text{Compression} = \frac{60484.5 - 60473.53 = 10.97}{3 (60473.53 \sin.^2 12^\circ - 60484.5 \sin.^2 9^\circ)} \text{ (Art. 7).}$$

$$\text{Log. } 60473.53 \dots\dots\dots = 4.7815653$$

$$\text{Log. } \sin.^2 12^\circ \dots\dots\dots = 2.6357578$$

$$\text{Log. } 2614.1 \dots\dots\dots = 3.4173231$$

$$\text{Log. } 60484.5 \dots\dots\dots = 4.7816441$$

$$\text{Log. } \sin.^2 9^\circ \dots\dots\dots = 2.3886648$$

$$\text{Log. } 1480.16 \dots\dots\dots = 3.1703089$$

$$3 (2614.1 - 1480.16) = 3 \times 1133.94 = 3401.82 \text{ and the compression} = \frac{10.97}{3401.82} = \frac{1}{310.1}.$$

*Example 2.*—Given 60484.5 fathoms for the length of a meridional degree, and 60856.5 fathoms the length of a degree perpendicular to the meridian in latitude  $12^\circ$  N. to find the compression.

$$\text{By Art. 8, compression} = \frac{60856.5 - 60484.5 = 372}{2 \times 60484.5 \times \cos.^2 12^\circ}$$

$$\text{Log. } \cos.^2 12^\circ \dots\dots\dots = 1.9808088$$

$$\text{Log. } 120969 \dots\dots\dots = 5.0826741$$

$$\text{Log. } 115739.8 \dots\dots\dots = 5.0634829$$

$$\text{Therefore } \frac{372}{115739.8} = \frac{1}{311.1} \text{ for the compression.}$$

*Example 3.*—Given in lat.  $13^\circ 34' 44''$  N. The length of a meridional degree 60491.46 fathoms, and the compression

$\frac{1}{310}$ , to find the length of a meridional degree at the equator.

By Art. 5,  $m = M(1 - 3e^2)$ , where  $M = 60491.46$ ,  $e = \frac{1}{310}$ , and  $s = \sin. 13^\circ 34' 44''$ .

Log. 3 . . . . .	= 0.4771213
Log. $\frac{1}{310}$ . . . . .	= $\bar{3}.5086383$
Sum . . . . .	= $\bar{3}.9857596$
Half sum . . . . .	= $\bar{2}.9928798$
Log. $\sin. 13^\circ 34' 44''$ . . . . .	= $\bar{1}.3706684$
Sum considered as a sin. . . . .	= $\bar{2}.3635482$
Cos. even therewith . . . . .	= $\bar{1}.9998842$
Its double . . . . .	= $\bar{1}.9997684$
Log. 60491.46. . . . .	= 4.7816940
Sum of two last . . . . .	= 4.7814624

Which corresponds to the 60459.2 fathoms, the required length of a meridional degree at the equator.

*Example 4.*—Given the compression  $\frac{1}{310}$  and the length of a meridional degree at the equator 60459.2 fathoms, to find the length of a degree perpendicular to the meridian at the equator.

By Art. 6,  $p = m \left( \frac{1 + 3e}{1 + e} \right) = 60459.2 \times \frac{313}{311}$ .

Log. 60459.2 . . . . .	= 4.7814624
Log. 313 . . . . .	= 2.4955443
Log. 311 ar. com. . . . .	= $\bar{3}.5072396$
Log. 60848. . . . .	= 4.7842463

Therefore 60848 fathoms is the length of a degree perpendicular to the meridian at the equator.

Otherwise, by retaining  $e^2$ , we have at Art. 4  $p = \frac{m}{(1 - e)^2} = \frac{m(310)^2}{(309)^2} = \frac{96100m}{95481}$

Log. $m$ . . . . .	= 4.7814624
Log. 96100 . . . . .	= 4.9827234
Log. 95481 ar. com. . . . .	= $\bar{5}.0200830$
Log. 60851.1 . . . . .	= 4.7842688

Which differs from the preceding value by 3.1 fathoms.

By Art. 4, compression  $e = 1 - \sqrt{\frac{m}{p}} = 1 - \sqrt{\frac{60459.2}{60848}} = 1 - .9968 = .0032 = \frac{1}{312.5}$ , instead of  $\frac{1}{310}$ . This is in a supposition that the length of a degree perpendicular to the meridian at the equator is 60848 fathoms.

*Example 5.*—Given the length of a meridional degree at the equator 60459.2 fathoms and the compression  $\frac{1}{310}$ , to find the equatorial diameter and polar axis.

By Art. 9,  $CK = \frac{vm}{1-e} = \frac{310 \times 57.29 \text{ \&c.} \times 60459.2}{309} = \text{half the polar axis.}$

Log. 310 . . . . .	= 2.4913617
Log. 57.29, &c. . . . .	= 1.7581226
Log. 60459.2 . . . . .	= 4.7814624
Log. 309 ar. com. . . . .	= 3.5100415
Log. 3475267 . . . . .	= 6.5409882

Therefore the polar axis is 6950534 fathoms, differing from Col. Lambton's measure by 358 fathoms.

By Art. 9,  $CH = \frac{vm}{(1-e)^2} = \frac{vm}{1-e} \times \frac{310}{309}$ .

Log. $\frac{vm}{1-e}$ , as above . . . . .	= 6.5409882
Log. 310 . . . . .	= 2.4913617
Log. 309 ar. com. . . . .	= 3.5100415
Log. 3486514 . . . . .	= 6.5423914

Therefore the equatorial diameter is 6973028 fathoms, differing from Col. Lambton by 360 fathoms.

Hence we have  $\frac{CH - CK}{CH} = \frac{11347}{3486514} = \frac{1}{309.99502}$ , according to example 5.  $\frac{a - c}{a} = \frac{22492}{6972668} = \frac{1}{310.00658}$  according to Col. Lambton.

*Example 6.*—Given the equatorial diameter 6973028 fathoms, to find the circumference of its corresponding circle.

Log. 2 . . . . .	= 0.3010300
Log. 3486514 . . . . .	= 6.5423914
Log. 3.141, &c. . . . .	= 0.4971499
Log. 21906414 . . . . .	= 7.3405713

The required circumference differing from Col. Lambton's by 1134 fathoms.

*Example 7.*—Given the lengths of a meridional and perpendicular degree at the equator 60459.2 and 60848 fathoms, to find the length of a degree at the pole.





By Art. 10,  $M = p \sqrt{\frac{p}{m}}$ , hence we have

Log. 60848 . . . . .	= 4.7842463
Log. 60459.2 . . . . .	= 4.7814624
Difference . . . . .	= 0.0027839
$\frac{1}{2}$ difference . . . . .	= 0.0013919
Log. 60848 . . . . .	= 4.7842463
Log. 61043.3 . . . . .	= 4.7856382

If  $p$  had been taken 60851.1, as found by the latter part of example 4, instead of 60848, a degree at the poles would be 61048 fathoms.

*Example 8.*—Given the length of a meridional degree at the equator 60459.2 fathoms and the compression  $\frac{1}{310}$ , to find the length of a meridional degree in latitude  $59^\circ$ .

By Art. 5, we have  $M - m = 3 \text{ } e \text{ } m \text{ } s^\circ$ .

Log. 3 . . . . .	= 0.4771213
Log. $\frac{1}{310}$ . . . . .	= $\bar{3}$ .5086383
Log. 60459.2 . . . . .	= 4.7814624
Constant log. . . . .	= 2.7672220
Log. $\sin.^2 59^\circ$ . . . . .	= $\bar{1}$ .8661312
Log. 429.88. . . . .	= 2.6333532
$m = 60459.2$	

$M = 60889.08$  fathoms the required length.

In this manner were the lengths of the meridional degrees, as given in the table, obtained, which agrees with Col. Lambton's table, calculated to every third degree.

*Example 9.*—Given the length of a degree perpendicular to the meridian at the equator 60848 fathoms, and the compression  $\frac{1}{310}$ , to find the length of a perpendicular degree in lat.  $75^\circ$ .

By Art. 5, we have  $P - p = p \text{ } e \text{ } s^\circ$ .

Log. 60848 . . . . .	= 4.7842463
Log. $\frac{1}{310}$ . . . . .	= $\bar{3}$ .5086383
Constant log. . . . .	= 2.2928846
Log. $\sin.^2 75^\circ$ . . . . .	= $\bar{1}$ .9698876
Log. 183.13. . . . .	= 2.2627722
$p = 60848.0$	

$P = 61031.13$  fathoms the required length.

In this manner might the lengths of the perpendicular degrees, as given in the table, be calculated, but I have preferred the following method :

By Art. 16,  $P' = \frac{M' - M}{3} + P$ , where  $M' - M$  represent the difference between any two contiguous meridional degrees (as shown in the table) and  $P'$ ,  $P$ , the lengths of two corresponding perpendicular degrees ; therefore, by beginning at the equator, or at the poles, the table may be easily formed ; for by example 4, the length of a perpendicular degree at the equator is given ; and by example 7, the length of a degree at the poles is known.

$$\text{Length of } 1^\circ = \frac{0.2}{3} + 60848.0 = 60848.1$$

$$2 = \frac{0.5}{3} + 60848.1 = 60848.3$$

$$3 = \frac{0.9}{3} + 60848.3 = 60848.6$$

$$4 = \frac{1.2}{3} + 60848.6 = 60849.0$$

$$5 = \frac{1.6}{3} + 60849.0 = 60849.5$$

$$6 = \frac{2}{3} + 60849.5 = 60850.2$$

and so on.

The length of a degree corresponding to  $3^\circ$  of latitude by the former part of this example is 60848.54 fathoms, and of  $6^\circ$  of lat. 60850.14 fathoms.

*Example 10.*—Given the equatorial diameter 6973028 and the polar axis 6950534 fathoms, to find the length of the elliptic quadrant.

By Mr. Barlow's tables, p. 292, we have  $c \{ 1 - (\frac{d}{2^2} + \frac{3d^3}{2^2 \cdot 4} + \&c.) \} = \text{elliptic meridian}$ , where  $1 - \frac{CK^2}{CH^2} = 1 - (1 - e)^2 = e(2 - e) = \frac{619}{(310)^2} = \frac{619}{96100} = d$ , and the  $\log. c = 7.3405713$  (example 6).

$$\text{Log. } d \dots\dots\dots = \bar{3}.8089672$$

$$\text{Log. } 2^2, \text{ ar. com. } \dots\dots = \bar{1}.3979400$$

$$\text{Sum } \dots\dots\dots = \bar{3}.2069072 \quad \text{Number} = .0016103$$

$$\text{Log. } 3 \dots\dots\dots = 0.4771213$$

$$\text{Log. } d^2 \dots\dots\dots = \bar{5}.6179344$$

$$\text{Log. } 2^2 \times 4^2 \text{ ar. com. } = \bar{2}.1938200$$

$$\text{Sum. } \dots\dots\dots = \bar{6}.2888757 \quad \text{Number} = .0000019448$$

$$\underline{\underline{.0016122448}}$$



Lat.	Degrees on the meridian.	Diff.	Degrees on the perpendicular.	Diff.	Lat.	Degrees on the meridian.	Diff.	Degrees on the perpendicular.	Diff.
59	60889.2	9.2	60992.2	3.0	79	61033.0	4.0	61037.1	1.3
60	60898.0	8.8	60995.2	3.0	80	61026.6	3.6	61038.3	1.2
61	60906.8	8.8	60998.1	2.9	81	61030.0	3.4	61039.5	1.0
62	60915.3	8.5	61000.9	2.8	82	61032.9	2.9	61040.5	1.0
63	60923.7	8.4	61003.8	2.9	83	61035.6	2.7	61041.4	0.9
64	60931.8	8.1	61006.5	2.7	84	61037.9	2.3	61042.2	0.8
65	60939.8	8.0	61009.2	2.7	85	61039.8	1.9	61042.8	0.6
66	60947.5	7.7	61011.8	2.6	86	61041.6	1.8	61043.4	0.6
67	60955.0	7.5	61014.3	2.5	87	61042.7	1.1	61043.8	0.4
68	60962.2	7.2	61016.7	2.4	88	61043.6	0.9	61044.1	0.3
69	60969.1	6.9	61019.0	2.3	89	61044.1	0.5	61044.2	0.1
70	60975.8	6.7	61021.2	2.2	90	61044.3	0.2	61044.3	0.1
71	60982.3	6.5	61023.4	2.2					
72	60988.4	6.1	61025.5	2.1					
73	60994.3	5.9	61027.5	2.0					
74	60999.8	5.5	61029.3	1.8					
75	61005.1	5.3	61031.1	1.8					
76	61010.0	4.9	61032.7	1.6					
77	61014.7	4.7	61034.3	1.6					
78	61019.0	4.3	61035.8	1.5					
$\frac{1}{2}$ sum of first and last degrees..					— 00751.7				
1219227.8					Length of elliptic quadrant.. = 5467656.4				

## ARTICLE VII.

*On the Action of Lime upon Animal and Vegetable Substances.*  
By Mrs. Ibbetson.

SIR,

Exmouth, Dec. 9, 1818.

I RETURN once more to my trench. I have, after three months' trial, again examined the plants that were placed in it; and though many have died in consequence of the extreme heat and want of moisture of this uncommonly fine season, yet most of those which are indigenous have thrown up shoots. Thus, confirming the former results, and again proving the folly of digging up weeds and then burying them to make manure, which was not produced in my first experiment, by even a three years' trial, though they were covered all that time with earth, and placed in the most favourable manner to produce it, if manure from decayed weeds could so hastily be procured by this means.

I now proceed to the next experiment, which indeed has furnished a result so singular and unexpected, that I have not yet got the better of my surprise. I mentioned in my first letter my intention of examining the truth of those generally received

axioms in agriculture ; viz. " that lime causes the decay of both animal and vegetable matter, thus producing constant food for fresh plants, and by this means nourishing the earth." I placed a piece of veal cut into slices, last summer, in mild lime, and to my great astonishment, it remained five months, two feet underground, without any smell or moisture, being, when discovered, *perfectly sweet*. The lime formed a sort of crust around it, which appeared to harden it, and render it impervious to air ; and I doubt not it would have continued, fresh and in the same condition, many months longer, if I had not taken it out. Having thus found by experiment, which has repeatedly been confirmed by subsequent trials with the same results, that lime, instead of destroying animal matters, preserved them, I was inclined to draw the same conclusion with respect to vegetables in general. In April last, I placed in lime a quantity of mutton, not in slices as before, but in one mass, weighing a pound ; and in another trough, in the same sort of lime, I put a quantity of vegetables, weeds as well as boughs of trees, annual and herbaceous plants, with some common heaths, with their hard roots. They remained nearly five months, and were then taken up for inspection. The meat, in spite of the extreme heat from the strong sunshine, to which the trough was in some measure exposed (being in a perfectly unshaded garden), remains as fresh as it was the first minute it was placed there ; but the vegetables were indeed in a very different state ; the lime lay on them perfectly loose, not coagulating or forming a crust, as on the meat, but most of them were not only rotted in the bark, but, in many cases, the wood was completely decomposed, and really almost in a state fit to furnish nutriment for other plants ; and I doubt not two months more would have reduced them sufficiently for forming new combinations suitable to the nourishment of each separate plant. There is, however, much difference in the manner of their decaying. The oak, the elm, the elder, and the heath, were completely decomposed, the wood in a powder, and black ; while the walnut and hazel nut, which in simple earth were more injured than any other sort of trees, were little touched, and certainly had not been long dead, since they had thrown out new buds, but without any bark to cover them. The plants, however, in general were decayed in a very remarkable degree. That the simple earth should preserve vegetables so completely as to keep them for the most part undecayed, and for the second year so thoroughly alive as to throw out fresh buds, and yet in the lime they should decay so entirely as to decompose the wood and reduce it to a wet powder, when a three years' trial in earth could not even discolour the wood, is a fact not a little curious and useful to agriculture. It would seem also very extraordinary, and contrary to what we should have previously expected, that animal matters can be preserved so perfectly in lime six or eight



months, and probably for a far longer period, when most vegetables were consumed completely in half that time. These facts will, however, lead us to a few results at present, as well as to future trials, and will enable us to explain and reconcile the opinion of many excellent agriculturists and chemists. It will also show why certain processes have always uniformly agreed and been advantageous when pursued. This may be the reason why lime so admirably (when enclosed with bog earth where it has first to reduce the undecayed vegetable matters, such as the limbs of trees and hard roots) decomposes plants so quickly and corrects the acid of the more decayed vegetable matter in bogs of older formation. It will also explain how completely possible it is in a piece of boggy heath, overrun with the heathy plants, to bring the ground into a perfect state of agricultural order and beauty in a short time, by the application of repeated quantities of lime freshly turned in. As hard as the roots are, a few months will, it seems, entirely decompose and reduce them to a black powder, which must of course be a fine manure, when lime is added to correct the acid, proceeding thence from the decomposed vegetable matter, as that vegetable matter first forms a powder and thick black liquid.

I have now placed the same collection of vegetables and boughs of trees in another trough to try the experiment once more, rendering the lime rather milder; while I am also adding another trench, with a mixture of half lime and half earth, to try their joint effects on vegetable matter, that I may ascertain whether the destruction of the vegetable is *caused* by the *passing* of quick lime into mild lime, or whether it depends immediately on the action of mild lime only on vegetable matter. Sir H. Davy says, in his admirable work on agriculture, that quick lime only improves *certain* soils, and that in the act of becoming mild; that is, in passing from pure to carbonate, it prepares soluble out of insoluble matters. Now the lime that I placed with both the animal and vegetable matter was exactly the same, equally slaked, and sufficiently so to be in fine powder. It is certain, however, that in that state it has not lost all its corroding power; for if water be thrown on it much heat is extricated; perhaps, by having decomposed the wood while in its first and most active state it was more easily completed by an after process, though no longer possessing any acid property.

But if it be by its heating or corroding quality, how comes it that it does not affect the animal as well as vegetable matter? There was quite lime enough to surround the plants in the same manner as the meat, since it lay on the boughs very thick; and though the bark immediately decayed, in no one instance did the lime collect round the plants, or coagulate around the stem. But to answer Sir H. Davy's question, whether it be the passage from quick lime to mild lime that effects the purpose of decomposition



the soil to supply by its decomposition carbonic acid, the magnesian limestone will remain long in its caustic state.

I perfectly agree with Mr. Dinsdale that nothing can be worse managed than all this part of farming in many parts of England. Farmers should make a sunk place out of the way of the house, as the Chinese do, where a large vat should be retained, covered with a wooden cover, into which the piggery might be discharged, and all the liquid mixture of the house emptied, and mixed with water, and thus thrown on the fields. It is certain that liquid goes three times as far as dry manure, and if the straw thrown in dirty lanes that makes such poor dung were thrown into the vat, it would soon afford double quantities of manure to every farmer; but it must be kept out of the way of the house; for I was myself a witness to a whole family being carried off by having a place for fattening hogs close to their house; but a vat may be managed with a degree of cleanliness that can be productive of no evil. When I take up my next trench, I shall continue the subject, and I hope to try the effect also of magnesian lime, and whether every species of animal manure is preserved.

I am, Sir, &c. &c.

AGNES IBBETSON.

## ARTICLE VIII.

*On the indigenous Plants in the North of England.*

By Mr. Winch.

(To Dr. Thomson.)

SIR,

Newcastle-upon-Tyne, Dec. 21, 1818.

DURING the course of the present year you admitted into the 11th volume of the *Annals of Philosophy* the first and second part of a tract on the distribution of plants through the northern counties of England; may I hope the remarks now transmitted will meet an equally favourable reception. Some meteorological observations should have been comprised in this paper, but I have not as yet ascertained in a satisfactory manner the temperature of the springs of water that issue into our deepest mines—a fact too interesting in an inquiry on subjects depending in a considerable degree on the mean temperature of the earth, as well as of the atmosphere, to be passed over in silence. These shall be communicated at a future opportunity.

Seven different species of fruit-trees ripen their fruit in the southern countries, which seldom or never do so in this latitude; these are the vine, the fig, the quince, the medlar, the walnut, the chestnut, and the mulberry. This may be ascribed in some measure to the prevalence of cold easterly winds during the spring months destroying the blossoms; to the low temperature of our autumns, which prevents the young wood from



sea-green fesque grass; *carex arenaria*, sea sedge, and occasionally *eryngium maritimum*, sea eryngo, and *juncus maritimus*, sea-rush; but to them may be added the following auxiliaries which are by no means exclusively the productions of the sea-shore: *triticum repens*, couch-grass; *galium verum*, ladies' bed-straw; *ononis repens*, rest harrow; *rosa spinosissima*, burnet leaved, or Scotch rose, with some compositæ, and a few other plants. The sea buck-thorn, *hippophæ rhamnoides*, is unknown here; nor are *dactylis stricta*, upright cocks-foot grass, *panicum dactylon*, creeping panic grass, or *juncus acutus*, great sharp sea-rush, all abounding on the shores of the mediterranean, to be met with; and the rare northern sedge, *carex incurva*, is also a stranger.

Though observations on organic remains may appear out of place among minutes intended to illustrate the geography of plants; yet it may not be amiss to remark that not one of the vegetables which have left impressions on our coal, &c. are known to exist at the present day. The casts which frequently occur in this coal field are those of the trunks of large trees imbedded in sand-stone, and mineralized by silex; but to what species they belonged, it is impossible even to conjecture, as no impressions of leaves remain: short thick stems resembling those of the genus *euphorbia*, mineralized by sandstone with iron pyrites, and coal-marks of ferns, like *osmunda regalis*, *filix mas*, *blechnum boreale*, gigantic reeds, rushes, cones, and a moss approaching to *fontinalis antipyrítica* in shale; fire-clay, sandstone, and especially in ironstone nodules. When erect, the *euphorbiæ*, reeds, &c. retain their proper shapes, but are always compressed when found in a horizontal position.

*Plants indigenous in England in Lat. 54°30', 55°30' North, arranged according to Jussieu's Method in natural Classes and Families.*

First Class.—Acotyledones.		Second Class.—Monocotyledones.	
1. Algæ . . . . .	164	1. Naiades . . . . .	6
2. Fungi . . . . .	314	2. Graminæ . . . . .	94
3. Hypoxylæ . . . . .	62	3. Cyperacæ . . . . .	56
4. Lichenes . . . . .	310	4. Typhacæ . . . . .	5
5. Hepaticæ . . . . .	45	5. Aroideæ . . . . .	2
6. Musci . . . . .	221	6. Juncæ . . . . .	17
7. Filices * . . . .	31	7. Asparagæ . . . . .	6
8. Lycopodiaceæ * . . . .	6	8. Alismacæ . . . . .	19
9. Rhizosperinæ * . . . .	1	9. Colchiacæ . . . . .	2
10. Equisetacæ * . . . .	6	10. Liliacæ . . . . .	12
Species . . . . .	1160	11. Judeæ . . . . .	2
		12. Archidæ . . . . .	20
		13. Hydrocharidæ . . . . .	1
		Species . . . . .	242

\* These four families are arranged by De-Candolle in the second class.





present century. For the first 12 years of that century, it was cultivated with indefatigable industry in almost every country of Europe. The result was a most important augmentation of chemical facts and the development of a new agent of almost unlimited power by which chemical decompositions might be brought about. Some addition has also been made to our electrical knowledge, though this department, as indeed might have been anticipated, has neither received so important nor so brilliant an increase as the chemical. The harvest has been already in some measure reaped; and the most active of the labourers have in a great measure left the field. A pause has ensued in the progress of galvanic discovery. The facts require classification and generalization. Whoever shall succeed in introducing a striking improvement in the theory will in all probability occasion a new stimulus to the activity of experimentalists, and give rise to the discovery of a new series of important facts.

At such a season as the present, a clear and candid history of galvanism cannot but be of considerable value. It will save the future theorist considerable trouble, by laying the facts before him within a short compass, and by pointing out the sources whence the requisite information on every branch of the subject may be obtained. The work before us deserves great praise as an historical sketch. It is written with much perspicuity, with unimpeachable candour; the accuracy where papers are referred to, written in the English or French language, is faultless, but the account of papers which have been published only in the German language is much less complete. Several very important papers are not noticed at all; and it is pretty obvious that the author has not had an opportunity of perusing others in the original language, but that he has been obliged to satisfy himself with the imperfect and garbled extracts which have occasionally appeared in the French periodical works. I allude particularly to the papers of Ritter, one of the most active experimenters, and, perhaps, the most bulky writer on galvanism that has hitherto appeared. He was a man of real genius, of the most extraordinary activity of mind, and the most unbounded ambition. This, together with his extreme youth, for he died at the age of 34, was the cause of the different extravagant doctrines which he from time to time advanced, and the inaccurate experiments by which these opinions were supported. Had he lived to a more mature age, he would have shaken off all these absurdities, and appeared in a light more befitting his genius and his industry; but with all these defects, his services were uncommonly great, and galvanism is indebted to him for a great deal which has been ascribed to subsequent writers. For this injustice, Ritter has in some measure to thank the obscurity of his writings and that mystical metaphysics with which they are interspersed. If any person who has studied Ritter's writings were to give us a simple statement of his experiments and



les nerfs ne seroient que des tuyaux, dont tout l'effet dependroit de la liqueur qu'ils contiennent."

After giving a concise account of Galvani's experiments and speculations, our author notices the publications of Valli and Fowler, the paper of Dr. Wells, the experiments of Humboldt, the letters and theories of Volta, and the paper of Fabroni, of Florence. Galvani ascribed the phenomena to the nervous fluid, Volta to common electricity, and Fabroni to the chemical action of the substances employed upon each other.

2. Volta ascribed the galvanic phenomena to the electrical action of the two metals upon each other. Bennet and Cavallo had shown that certain metals, when placed in contact, and then separated from each other, are found in different states of electricity; the one plus, and the other minus. Volta conceived that the evolution of electricity in galvanic experiments was owing to this law. The zinc plate became plus, and the silver or copper plate minus. He conceived that by increasing the number of plates with a liquid conductor between each pair, the electricity might be increased at pleasure, and the effects accordingly made as conspicuous as could be desired. This fortunate idea gave rise to the Voltaic pile, which was found by the inventor capable of giving shocks, and of producing more violent convulsions than a single pair of plates could do.

Volta's description of the galvanic pile was published in the *Phil. Trans.* for 1800. Messrs. Nicholson and Carlisle were the first to try the properties of this new instrument. They speedily discovered that it possessed the power of decomposing water, that the oxygen was evolved from the wire attached to the zinc end of the pile, which they found to be the positive wire; while the hydrogen was evolved from the wire attached to the copper end of the pile, or the negative wire.

Mr. Cruickshanks, of Woolwich, substituted the trough for the original pile of Volta. This was found a much more convenient modification of the apparatus than the original contrivance of Volta. It has, therefore, taken its place, and been gradually improved into the state in which it is at present employed. Cruickshanks observed several of the decompositions produced by the Voltaic trough, and ascertained the laws of these decompositions. These decompositions were carried still further by Henry, and particularly by Davy.

Cruickshanks observed that liquids destitute of oxygen are incapable of transmitting the galvanic energy, while it is transmitted in his opinion by every liquid that contains that principle. Col. Haldane found that the apparatus ceased to act when plunged in water, or when placed in the vacuum of an air-pump. Azotic gas had the same effect as a vacuum; while oxygen gas made it act more powerfully. From these facts, and from a series of experiments by Dr. Wollaston, it was

concluded that the energy of the pile depends upon the chemical action between the liquid conductor and the metals. This liquid must be such as to be capable of oxidizing and dissolving one of the metals; while it is incapable of acting upon the other. The galvanic energy continues as long as the oxidizement of the metal; but when this oxidizement is at an end, the energy likewise ceases to produce any sensible effect. Dr. Wollaston even went so far as to infer, that the evolution of electricity in the common electrical machine depends upon the oxidizement of the amalgam attached to the rubber, and this oxidizement he considered as originating from the oxygen of the surrounding atmosphere. Accordingly he found that the electrical machine lost its energy when inclosed within the exhausted receiver of an air-pump; but when the Rev. Mr. Wollaston, at that time Professor of Chemistry at Cambridge, repeated this experiment, the result was different.

It would appear from the present state of our knowledge that the essence of the galvanic battery is a series of metallic plates so placed that one side is oxidized and dissolved by a liquid, while the other side of each plate is completely screened from the action of the liquid. The plates must all be connected with each other by means of conductors of electricity. The only use of the copper plates in the battery seems to be to protect one side of the zinc plates from the chemical action of the liquid. In what way it produces this effect is not very obvious, though the fact is undoubted. If we were to adopt the notion of Berzelius, and, I believe, of Davy, that chemical affinity and the electric forces are merely different names for the same thing, we might be able to form some notion how the effect is brought about; though not without adopting a theory of electric action very different from the one hitherto adopted. Let us suppose that dilute nitric acid is essentially negatively electric. In that case it will act only on bodies which possess positive electricity, and its action will be most powerful the greater the intensity of the positive electricity in the substance on which it acts. Now it appears from the experiments of Bennet, Cavallo, and Volta, that when a plate of zinc is laid upon a plate of copper, if the two plates be separated, we find the zinc in a plus, and the copper in a minus electric state. If the zinc by this contact becomes decidedly positive, and the copper decidedly negative, then it follows that the diluted nitric acid will act with energy upon the zinc, and not at all upon the copper.

Sir H. Davy's pile, composed of one metal and two liquids, one of which acted on the metal, while the other did not, shows very obviously that two metals are merely necessary in the galvanic battery in order to screen the one side of the most easily oxidizable metal from being acted on; while the action goes on freely on the other side. The sulphuret in Sir H.



Davy's pile served merely the purpose of screening one side of the zinc, while the other side was freely exposed to the action of the nitric acid.

Trommsdorf's discovery of the efficacy of large metallic plates in producing combustion was the next step in the improvement of the apparatus. This discovery was verified by Fourcroy, Thenard, and Vauquelin. These gentlemen found that the electrical action of the battery was not increased by the increase of the size of the plates, but by the increase of the number of pairs; but the chemical action, as far as combustion is concerned, and as far as the decomposition of those bodies which are not very difficult of decomposition, is increased with the size of the plates.

3. But the most important addition made to our knowledge of galvanic decomposition is contained in a paper written by Hisinger and Berzelius, and published in the first volume of Gehlen's Journal, in 1803. They ascertained by a copious induction that when bodies are decomposed by galvanism, *oxygen* and *acids* are accumulated round the *positive* pole; while *hydrogen*, *alkalies*, *earths*, and *metals*, are accumulated round the *negative* pole. Sir H. Davy verified this law of Berzelius with much sagacity, in a paper which constitutes one of the finest examples of experimental investigation any where to be met with. He showed that the acid and the alkali, which were affirmed to be formed when water was decomposed by galvanism, and to accumulate round the positive and negative wires, respectively owed their origin to the decomposition of some common salt found either in the water or in the vessel in which the water was kept; and that when perfectly pure water is used, and vessels quite free from all contamination of salt, the water is decomposed without the evolution or formation of any thing, except oxygen and hydrogen. Davy, after verifying Berzelius's discovery, and tracing the way in which the decomposed constituents penetrate to the respective poles, went a step further. In his opinion, every body in nature possesses a certain permanent electric state. Their affinity for each other depends upon this state. If they be in the same state, they have no affinity for each other; if they be in opposite states, they have an affinity, and this affinity is the greater the more intensely opposite the electrical states of the two bodies are. If, therefore, we wish to decompose a compound, we have only to bring its constituents to the same electric state, they will in consequence of this separate of themselves. Now galvanism affords us this means. We have only to put it in practice and it will enable us to decompose compounds, the constituents of which are so intimately combined as to have resisted all former efforts to separate them. This theory was applied with the happiest success by Davy to the decomposition of the fixed alkalies, and even of the alkaline earths. Boracic acid was afterwards decomposed by Gay-Lussac and Thenard; and sufficient evidence was brought by Davy, Berzelius, and Stromeyer, that silica is a compound of oxygen, and a combustible basis. Such



## ARTICLE X.

*Proceedings of Philosophical Societies.*

## ROYAL SOCIETY.

June 10.—A paper, by Capt. H. Kater, was begun, entitled “An Account of Experiments for determining the Length of the Pendulum vibrating Seconds at the principal Stations of the trigonometrical Survey.”

June 17.—Capt. Kater’s paper was continued.

June 24.—Capt. Kater’s paper was concluded. The author commenced by noticing the reasons which induced him to undertake the experiments forming the object of his present report; and afterwards proceeded to describe the apparatus employed. The operations at each station were then minutely detailed, the results stated at length, and illustrated by numerous tables.

For the latitude of London, the length of the pendulum vibrating seconds on the scale forming the basis of the trigonometrical survey was stated to be

	39·13722 inches.
For the latitude of Unst . . . . .	39·16939
Portsoy. . . . .	39·15952
Leith-fort. . . . .	39·15347
Clifton. . . . .	39·14393
Arbury Hill . . . . .	39·14043
Shanklin Farm . . . .	39·13407

The calculations of the latitude of each of these stations was then given at length. The latitude of Arbury Hill, which has been supposed to be erroneous, was found by Capt K. to be correct. The whole was concluded with some observations on the figure of the earth

It appeared from this interesting report, to which it is impossible to do justice by merely hearing it read, that, excepting the allowance for the height above the level of the sea, the error in the vibrations of the seconds pendulum at any particular station did not amount to  $\frac{1}{10}$ th of a vibration, which is about equal to the 400,000 part of the length, consequently that the amount of the gravitation could be determined to this degree of accuracy. Now this is so near as to indicate the different degrees of density of the materials constituting the substrata of the different stations in a country selected for experiment. Hence Capt. K. concluded that minute differences in density, indicated by the pendulum, are often to be referred to irregularities of attraction: thus the sudden increase of gravitation at Arbury Hill was supposed by the author to be produced by the granite existing in Mount Sorrel, in Leicestershire.

Capt. Kater stated that he had learned with pleasure that M. Biot’s results (the details of which are not yet published),



A paper was read from the Hon. W. I. H. I. Strangways, on the strata in the brook Pulcova, near the village of Great Pulcova.

The Pulcova is a small stream which passes with a number of angular sweeps through a deep ravine, the sides of which exhibit a very singular disposition of their strata, which it is the object of this paper to describe.

In the upper part of the valley, we find only blue clay covered by the alluvium; as we descend, we meet with limestone lying conformably above the clay, and afterwards portions of a green or dark coloured clay, which rise up between the blue clay and the limestone, the strata being much contorted, and dipping in various directions. The green clay is interstratified with thin seams of a whitish sand, so as to produce a striped appearance in its sections. In some parts these alternations of green and white sand rise up like the summit of a rounded hillock; in other parts they are perpendicular, and occasionally they are twisted in the most irregular manner. The beds of limestone also vary much in their direction; sometimes they are nearly perpendicular; they dip in opposite directions in different parts, and they suffer various contortions. For a considerable space the three strata, during all their contortions, are conformable to each other; but we afterwards arrive at a spot where the limestone is but slightly inclined, and seems to abut abruptly against the beds of clay, which are here nearly vertical. The position of the limestone in relation to that of the green clay is, however, rendered somewhat doubtful, in consequence of the tendency which the former rock has to divide itself into cubical fragments, which renders the actual direction of the strata somewhat uncertain. Some portions of the limestone consist of a number of strata, which are variously coloured; and in consequence of its tendency to divide into cubical fragments in certain parts presents the appearance of a beautiful tessellated pavement. The paper was accompanied by a map, and a number of illustrative drawings.

An abstract of a letter from Dr. Nugent, of Antigua, to the President, was read, accompanying some specimens of the Barbuda limestone, and containing some remarks on the geology of that island, and of Antigua.

Dr. Nugent describes Barbuda as consisting of a hard, level, limestone, with scarcely any vegetable mould upon it, a good deal of brush wood and copse growing in the crevices, and being altogether about 20 miles long, and 13 or 14 broad. The limestone is supposed to be of the same formation with that of Antigua. The fossils in the Barbuda rock at first view appear to be recent, but this idea is inconsistent with the connexion between the Barbuda limestone and that of Antigua in which so many siliceous fossils occur.

The author remarks that the more mountainous parts of Antigua consist of trap rocks, on which rests a series of strati-





*regulus* was always brittle. By adopting a different method, I have at last succeeded; and as a description of the process in which this was effected will add one more amusing experiment to those which have already taken place with the *gas blow-pipe*, and enable every one of your chemical readers to obtain *nickel* in a state of perfect purity and malleability, without the smallest difficulty, I will now relate it as briefly as possible.

Having dissolved the brittle *regulus*, sold under the name of *nickel* by Mr. Knight, of Foster-lane, London, in highly concentrated *nitric acid*, and afterwards evaporated almost all the acid, leaving only a small residue of the solution, in its concentrated state, for crystallization, I obtained hard green crystals of the *nitrate of nickel*. Then placing some of these crystals within a cavity scooped in a stick of *charcoal*, I exposed them to the flame of the *gas blow-pipe*. Presently the whole of the *nitrate* upon the *charcoal* became liquid as water, and in this state, being still carefully supported and exposed to the flame of the *gas blow-pipe*, all moisture was driven off; a dry crust remaining upon the *charcoal*, which, by further exposure to the flame, became a dark slag. This being now removed, and placed in another *charcoal crucible*, as before, and again exposed to the flame of the *gas blow-pipe*, ran together in a state of fusion, and was held boiling until it ultimately appeared in the form of a metallic bead, with a very tarnished surface. When it became cold, it was highly magnetic; and upon being cut with a file, it exhibited the whiteness of silver. It was then beat out upon an anvil by violent shocks of a large hammer, being perfectly malleable. Wishing to extend the surface of it, and fearing to lose it from the anvil, Professor Cumming, who examined it, placed it within one of Mr. Knight's beautiful steel mortars, in which it was compressed into a circular *lamina*. This I have sent to you for your examination. I have also added another result of fusion before the *gas blow-pipe*, which is also worthy of your notice. It is, perhaps, a similar result to that exhibited by *wood-tin* after fusion; of which you before published an account; namely, a crystal of *titanium oxide*; which, in the part fused, not only exhibits a considerable degree of metallic lustre without being cut by a file; but, if you will examine the melted surface with a powerful lens, you will perceive traces of that dendritic crystallization by which metals in cooling are often characterized. However, as I suspect, from its retaining its metallic appearance unaltered (which is about equal to that exhibited by *selenium*), that it is still in the state of an *oxide*, I request that you will have the goodness to settle this point. It owes nothing of its metallic aspect to any contact with *carbon* during fusion; not having been placed upon *charcoal*, but being exposed *per se* to the action of the *gas blow-pipe*.

I remain, &c. &c.

Cambridge, June 21, 1819.

EDWARD DANIEL CLARKE.



green, refuses to yield any more crystals of sulphate of nickel. By evaporating it to the requisite consistency, and then setting it aside, a very abundant deposit is made of an apple-green salt, which adheres very firmly to the evaporating dish. I took this matter at first, from its colour, to be arseniate of nickel; but I found it on examination to be a double salt, consisting of sulphate of nickel and arseniate of nickel united together. I endeavoured to get rid of the arsenic acid by dissolving the salt in water and causing a current of sulphuretted hydrogen gas to pass through it as long as any precipitate appeared. By this method I threw down a great deal of arsenic; but on filtering and evaporating the liquor, it was still converted into an apple-green matter, and of course contained arsenic. I found that when this salt was dissolved in water, the liquid became opaque, owing to a quantity of arsenious acid which separated from the salt. The liquid being now filtered to get rid of the arsenious acid, and properly evaporated, yielded a new crop of crystals of sulphate of nickel. These crystals continued to be deposited as long as a single drop of the liquid remained unevaporated.

By this method may the whole of the nickel in the speiss be obtained in the state of sulphate of nickel. This sulphate is quite free from arsenic or arsenious acids; for the presence of these acids prevents sulphate of nickel from crystallizing. But for greater security, I dissolve the sulphate of nickel in water, and crystallize a second time.

The pure sulphate of nickel thus obtained is dissolved in water, and decomposed by carbonate of soda. The carbonate of nickel, when well washed and dried, is a light-green coloured powder. I make it up into balls with a little oil; enclose these balls in a charcoal crucible, which is put into a hessian crucible, the mouth of which is covered and luted. It is now exposed to the greatest heat that I can raise in a melting furnace for two hours. By this process, I have always obtained a button of pure nickel in the metallic state.

The nickel thus obtained is hard, but malleable, and very obedient to the magnet. I think it contains a certain proportion of carbon in combination with the nickel. The button is usually surrounded with a thin dark shining cuticle, which I take to be a carburet of nickel.

### III. *Acetate of Ammonia.*

It does not appear that chemists have hitherto hit upon a method of obtaining this salt in crystals; at least I find no traces of any such method in the latest chemical works which I have seen, nor indeed any addition to the facts known respecting this salt more than 30 years ago. I conceive, therefore, that it will be acceptable to practical chemists to be put in possession of a method of crystallizing this salt with as much facility as any





plants which delight to vegetate in a thirsty soil. M. Damart informs us that the cactus tuna, and other species of cactus which vegetate in a similar soil in South America, produces a gum of exactly the same properties with the gum of Bassora. —(Jour. de Pharmacie, Ibid. p. 184.)

#### VI. Sulphate of Lime.

Some chemists have lately recommended the following method of determining the quantity of lime in mineral waters: Throw down the lime by oxalate of ammonia; wash the precipitate, and dry it; expose it to a red heat in a platinum crucible, which will convert it into carbonate of lime; pour an excess of sulphuric acid over this carbonate, evaporate to dryness, and expose the dry mass to a red heat. It will now be sulphate of lime. Weigh it;  $\frac{3.625}{8.625}$  of the weight indicates the lime.

This method may, perhaps, answer when the lime amounts only to a grain or two; but if its quantity amounts to 40 or 50 grains, or probably even to 15 or 20 grains, it is not possible to combine the whole of it with sulphuric acid by a single process. Hence the lime, when estimated in this way, will always be below the truth. ✓

The simplest way to estimate the quantity of lime in oxalate of lime is to expose the dry oxalate to a white heat in a platinum crucible. By this process, it is converted into quick lime, and nothing more is necessary than to weigh it in order to determine its quantity. If an experimenter has not the means of producing a sufficiently high temperature to reduce the oxalate to quicklime, he may proceed in the following way: Expose the oxalate of lime to a red heat; dissolve the carbonate of lime thus obtained in muriatic acid; add to the solution a quantity of liquid sulphate of ammonia sufficient to decompose the whole muriate of lime formed; evaporate to dryness; and keep the saline mass for half an hour in a red heat. It is now pure sulphate of lime, from which the quantity of lime may be determined.

#### VII. Zircon.

This mineral has been analyzed by Klaproth and Vauquelin, and found by both of these chemists to be a compound of silica and zirconia with a small portion of oxide of iron. Neither of them noticed the presence of any alumina. I find, however, that alumina is a constant ingredient. I analyzed a quantity of crystallized zircons last winter by fusing them with thrice their weight of potash, and treating the fused mass in the usual manner. After separating the zirconia, I dissolved that earth in sulphuric acid, and concentrated the solution, after adding to it a certain quantity of sulphate of potash. This mass, which was not quite liquid, but contained a mixture of sulphate of zirconia in the state of a white powder, being set aside for some



The spring is situated, as I understand, about midway between Newhaven and Rottingdean, at an elevation of about 15 or 16 feet above the level of the sea at high water mark. It issues from between the cliffs or fissures of the chalk in small streams, and these, when united, pour forth about 20 to 25 gallons in the hour, the chalk about the place is every where tinged with ochrous deposit. Its temperature, as it issues, is 65° Fahr. and remains constantly the same. When I received it, there was a deposit of brownish colour, which proved on examination to be oxide of iron. Its specific gravity, at the temperature of 60° Fahr. was 1.076; it is slightly acidulous, changing the colour of litmus paper both before and after boiling, by which operation it deposits a further portion of oxide of iron, and also a little lime. Reagents show it to contain the following substances in solution :

Oxide of iron.  
 Alumina.  
 Muriatic acid.  
 Sulphuric acid.

Lime.  
 Carbonic acid.  
 Soda.

This last substance I will not be quite certain of; but I expect shortly to be able to make a more perfect analysis, and to give a better account of its situation, which is of some importance, as I expect it is not far distant from the spot where the native alumina or subsulphate is found. I remain, dear Sir,

Very sincerely, yours,

JOHN THOMAS COOPER.

*X. Vindication of the Description of the horizontal Moon given by Dr. Clarke in his Travels.*

(To Dr. Thomson.)

SIR,

A correspondent, who signs himself S. in the number of your *Annals* for June, has called upon Dr. E. D. Clarke to explain a passage in his travels which appears to him to involve an impossibility. It is that in which a description is given of the appearance of the *horizontal moon*; as observed on the road from Tornea to Kiemi. The moon appearing to be of an oval form is there said to have been observed as if it had been surrounded by a luminous belt, like that of the planet *Saturn*. This *deceptio visus* Dr. Clarke attributes to the accidental position of a body of clouds; which he says were "collected in the form of a ring around the moon." Having used this word *form*, one would think all ambiguity was thereby done away; as the description evidently refers to an *appearance* and not to a *reality*; but your correspondent believing an *actual belt* to be intended, gravely undertakes to prove its *impossibility*; and this surely is too ludicrous to merit any further notice.

X.



similar water known, except the water of the Dead Sea, analyzed by Dr. Marcet some years ago, which contains even a greater proportion of saline contents.

It may be proper to observe, that there is a little discrepancy in the results obtained, 500 gr. of the water being estimated, from other experiments, to yield, when evaporated to dryness, only 111.5 gr. of salts. This difference is probably partly to be referred to the different degrees of desiccation employed, and partly to the smallness of the quantities operated upon, Dr. M. having originally possessed only between 200 and 300 gr. of the water.—(Abstracted from a paper entitled “On the Specific Gravity and Temperature of Sea Waters in different Parts of the Ocean, and in particular Seas, with some Account of their saline Contents,” by Alex. Marcet, M.D. F.R.S. &c.)

## XII. Temperature of June.

The temperature of the month of June, 1819, was uncommonly low at Glasgow. The oats made very little progress during the whole of the month; for they were nearly as far advanced at the end of May as in the beginning of July. The only warm days were June 17 and 18, the temperature of which was as follows:

THERMOMETER.		
	Lowest.	Highest.
June 17 . . . . .	49 . . . . .	71.50
18 . . . . .	55 . . . . .	70.00
Mean . . . . .	52 . . . . .	70.75
Mean of both . . . . .		62.375

During the rest of the month, the thermometer seldom was higher than 60° or 62°, and seldom lower than 50° or 48°. The mean for the month was as follows:

Lowest.	Highest.	Mean of both.
49.27 . . . . .	62.43 . . . . .	55.85

This is at least 7° below the usual mean of the month of June. The wind veered very much; but blew nine-tenths of the month from the south-west.

The barometer was on the whole high; the mean for the month, between the hours of 12 and 2 o'clock, the height of the mercury being reduced to what it would have been supposing its temperature to have been always 32°, was as follows:

29.7107 inches

at a height of about 100 feet above the level of the sea.

The month was rather wet, though not remarkably so. There were five days during which it rained the greatest part of the day; and there were 15 other days upon which showers fell; so that there were only 12 days of the month quite free from rain.





XIV. *Vaccination in Denmark.*

For the last eight years not a single case of small-pox has occurred in the dominions of the King of Denmark. The whole inhabitants have been vaccinated. Here is one good effect which has resulted from the arbitrary power of the King of Denmark. Between 1752 and 1762 the small-pox carried off in Copenhagen alone 2644 victims; from 1762 to 1772, it carried off 2116; from 1772 to 1782, 2233; from 1782 to 1792, 2735; but from the introduction of vaccination in 1802 to the end of 1818 only 158 persons have died of the small-pox; namely, 1802, 73; 1803, 5; 1804, 13; 1805, 5; 1806, 5; 1807, 2; 1808, 46; 1809, 5; 1810, 4; 1811, 0; 1812, 0; 1813, 0; 1814, 0; 1815, 0; 1816, 0; 1817, 0; 1818, 0.

XV. *New Instrument.*

Mr. Perkins, of Philadelphia (whom the newspapers have announced as on his passage to England, in order to submit to the Directors of the Bank of England a specimen of bank bills which defy forgery), has invented an instrument, called the *Bathometer*; which is intended to show, by the compressibility and elasticity of water, *the depth of the sea*. He is said to have produced a pressure of water in a confined column equal to that of more than 200 atmospheres, or upwards of 3000 pounds to every square inch of surface; being equal to the pressure of 6,400 feet in fresh water. Mr. Perkins intends to prepare a graduated scale, showing the exact degree in which water is actually compressible.

XVI. *Death of Professor Playfair.*

John Playfair, Esq. F.R.S. Lond. and Edinb. &c. Professor of Natural Philosophy in the University of Edinburgh, died on Tuesday, July 20, at his house in Forth-street, Edinburgh. He was the son of Dr. James Playfair, author of a *System of Chronology* of great reputation. The Professor was supposed to be the principal conductor of the scientific department of the *Edinburgh Review*. The works which he has left behind him are: *Elements of Geometry*, 8vo. 1796; second edition, 1804; *Illustration of the Huttonian Theory of the Earth*, 1802; a *Letter to the Author of the Examination of Professor Stewart's Statement*, 8vo. 1806; a *Complete System of Geography, Ancient and Modern*; and detached papers in various periodical publications.

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ERRATUM in the present Number.

Page 90, line 28, for 5·917, read 6·4367.



## Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Siz's.
June		Inches.				Feet.		
1	Morn....	29.022	56°	51°	SW		Fine	46½
	Noon....	29.610	64	35	WSW		Cloudy	64
	Even....	29.607	56	42	SW by W		Cloudy	53½
2	Morn....	29.623	57	73	SSW		Rain	66
	Noon....	29.602	65	52	SW by W		Fine	49½
	Even....	29.565	59	50	SSW		Fine	64½
3	Morn....	29.564	57	52	SSW		Fine	55½
	Noon....	29.560	64	40	SSW		Cloudy	67½
	Even....	29.534	59	43	S by W		Cloudy	45
4	Morn....	29.478	60	59	SW		Showery	66½
	Noon....	29.480	63	47	S		Showery	48
	Even....	29.395	59	77	S		Rain	66
5	Morn....	29.583	54	52	WNW		Fine	52
	Noon....	29.619	65	34	W		Fine	63½
	Even....	29.620	60	41	WSW		Very fine	49½
6	Morn....	29.586	54	58	SSW		Foggy	68
	Noon....	29.510	66	33	S		Fine	50
	Even....	29.400	57	35	SE		Cloudy	66
7	Morn....	29.164	58	55	SW by S		Cloudy	63½
	Noon....	29.164	62	42	WSW		Cloudy	49½
	Even....	29.200	55	55	SW		Cloudy	67½
8	Morn....	29.162	58	51	SSE		Cloudy	52
	Noon....	29.144	65	44	S		Showery	68
	Even....	29.118	60	43	SSE		Fine	50
9	Morn....	29.158	59	53	S by W		Fine	66
	Noon....	29.158	67	35	SSW		Fine	48
	Even....	29.160	56	40	SW		Fine	61
10	Morn....	29.254	56	47	W		Fine	46½
	Noon....	29.284	63	35	SW		Showery	67½
	Even....	29.372	54	49	SW		Showery	49½
11	Morn....	29.521	58	47	W by S		Fine	63
	Noon....	29.534	60	41	W		Hail	46
	Even....	29.534	53	51	W		Showery	61
12	Morn....	29.540	56	50	W by S		Fine	46½
	Noon....	29.540	60	44	Var.		Showery	61
	Even....	29.577	54	61	W		Very fine	46½
13	Morn....	29.620	56	40	S		Very fine	67½
	Noon....	29.594	66	35	SSW		Very fine	49½
	Even....	29.552	56	42	SSW		Very fine	63
14	Morn....	29.569	59	42	W		Fine	49
	Noon....	29.510	60	54	W by S		Cloudy	61
	Even....	29.456	55	57	S by W		Rain	49
15	Morn....	29.305	55	71	SW		Rain	61½
	Noon....	29.300	58	52	W by S		Fine	46
	Even....	29.343	54	57	WNW		Very fine	60½
16	Morn....	29.469	53	40	NW by W		Fine	47½
	Noon....	29.490	59	37	NW		Cloudy	65
	Even....	29.580	54	51	NW		Fine	52
17	Morn....	29.632	55	56	N		Showery	60½
	Noon....	29.643	63	42	NE		Cloudy	
	Even....	29.670	56	48	NE		Cloudy	
18	Morn....	29.588	54	70	NNW		Sm. rain	
	Noon....	29.583	56	78	N		Rain	
	Even....	29.605	59	72	NNE		Cloudy	





## ARTICLE XIII.

## METEOROLOGICAL TABLE.

1819.	Wind.	BAROMETER.			THERMOMETER.			Hygr. at 9 a. m.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
5th Mo.									
May 16	S E	30.08	30.02	30.050	72	37	54.5	59	D
17	N E	30.02	29.86	29.940	75	40	57.5	59	
18	S W	29.86	29.70	29.780	69	50	59.5	60	48
19	N E	29.70	29.63	29.665	65	54	59.5	86	24
20	S W	29.63	29.59	29.610	63	51	57.0	71	40
21	S E	29.66	29.49	29.575	62	42	52.0	76	4
22	S E	29.90	29.66	29.780	70	39	54.5	70	
23	E	29.91	29.90	29.905	77	50	63.5	65	27
24	E	29.94	29.91	29.925	63	49	56.0	72	4
25	N E	29.91	29.90	29.905	65	47	56.0	72	
26	N E	29.92	29.91	29.915	60	40	50.0	70	
27	N E	29.91	29.87	29.890	63	42	52.5	63	
28	N E	29.93	29.89	29.910	60	33	46.5	60	—
29	N E	30.02	29.93	30.025	60	31	45.5	60	
30	N W	30.09	30.02	30.055	64	44	55.0	55	9
31	S W	30.13	30.09	30.110	64	46	55.0	62	5
6th Mo.									
June 1	S W	30.10	30.09	30.095	70	53	61.5	64	C
2	S W	30.10	30.06	30.080	70	47	58.5	67	
3	S W	30.06	30.00	30.030	72	56	64.0	61	
4	S W	30.06	29.95	30.005	72	45	58.5	71	9
5	S W	30.10	30.07	30.085	72	45	58.5	63	
6	S W	30.07	29.70	29.885	75	53	64.0	59	1
7	S W	29.72	29.68	29.700	66	47	56.5	65	
8	S E	29.66	29.63	29.645	70	46	58.0	62	1
9	S	29.72	29.67	29.695	75	51	63.0	62	0
10	S W	29.97	29.72	29.845	72	46	59.0	59	25
11	W	30.04	29.97	30.005	70	47	58.5	63	
12	S W	30.09	30.04	30.065	70	37	53.5	60	2
13	S E	30.10	30.04	30.070	70	48	59.0	59	
14	S W	30.04	29.85	29.945	71	49	60.0	61	3
		30.13	29.49	29.899	77	31	56.90	64	2.02

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## METEOROLOGICAL TABLE.

1819.	Wind.	BAROMETER.			THERMOMETER.			Hygr. at 9 a.m.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
6th Mo.									
June 14	S W	30.04	29.85	29.945	71	49	60.0	03	61
15	S W	29.97	29.83	29.900	63	42	52.5	33	65
16	N W	30.12	29.97	30.045	62	45	53.5	02	61
17	N W	30.13	30.07	30.100	72	52	62.0	01	63
18	N W	30.14	30.05	30.095	67	52	59.5	45	82
19	N W	30.19	30.14	30.165	72	47	50.5		66
20	N W	30.19	30.18	30.185	72	45	58.5		61
21	N W	30.18	30.09	30.135	73	49	63.5		59
22	N W	30.09	30.08	30.085	63	55	61.5		59
23	N W	30.08	29.94	30.010	72	52	62.0		59
24	S W	29.94	29.81	29.875	63	56	59.5	05	66
25	S W	29.93	29.72	29.825	66	55	60.5	—	75
26	S	29.74	29.69	29.715	67	48	57.5	—	69
27	Var.	29.74	29.73	29.735	65	50	57.5	30	70
28	N W	29.86	29.73	29.795	60	45	52.5	23	64
29	N W	29.86	29.79	29.825	70	51	62.0		63
30	W	29.86	29.77	29.815	72	46	59.0		67
		30.19	29.69	29.955	78	42	58.88	1.42	65

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Sixth Month.*—14. Cloudy. 15. Rainy. 16, 17. Cloudy. 18. Wet, a. m. 19—23. Fine days. 24. Morning, overcast: drizzling rain. 25, 26. Cloudy. 27. Frequent heavy showers: at sun-set, strong shadows projected from behind a *Cumulus* or two, amidst a wild-looking sky: the clouds opposite the setting sun tinged to a very full yellow. 28. Cloudy morning: a confused sky, exhibiting a mixture of *Cirrus*, *Cirrocumulus*, *Cumulus*, &c.: about 11, a. m. some hail ushered in a thunder shower: the lightning was vivid at Stratford, and the thermometer fell 8° during the shower. Other thunder showers succeeded, till one,

p. m. when there was a very heavy storm of large hailstones, which nearly covered the ground. This was attended with vivid lightning to the southward, and thunder at a small distance.  
29. Overcast. 30. Fine.

## RESULTS (of the half period).

Winds Westerly, and chiefly North-west.

Barometer: Greatest height ..... 30·19 inches.  
Least. .... 29·69  
Mean height. .... 29·995

Thermometer: Greatest height ..... 78°  
Least. .... 42  
Mean height. .... 58·88

Rain at Stratford ..... 1·42 inch.

Rain at Tottenham ..... 1·63

Mean of the Hygrometer. .... 65°

*Tottenham, Seventh Month, 20, 1819.*

L. HOWARD.

\*\*\* The half table now published brings up these observations to the close of the half year. In future, to facilitate the comparison of the daily observations and monthly results with those of other registers, they will be published for every calendar month in the usual way. At the same time, to preserve the original intention, which has succeeded hitherto, of bringing occasionally into view the periodicity of atmospherical changes, the mean results will be inserted in the columns where they now stand daily, at such intervals only as experience has demonstrated to be best adapted for the purpose of forming an annual table, which it is intended in future to publish as a supplement to each year's observations. The whole of the materials for the tables are at present derived from the register kept (under the care of my friend John Gibson) at the laboratory, Stratford, where the due position and accurate adjustment of the instruments have been recently attended to by myself. This arrangement has become necessary to me, as the second volume of the *Climate of London* will occupy, until it leaves the press, nearly the whole share of attention which I can personally give to the subject.

L. H.

# ANNALS

OF

## PHILOSOPHY.

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SEPTEMBER, 1819.

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### ARTICLE I.

*A Tribute to the Memory of the late Thomas Henry, F.R.S. &c. &c. President of the Literary and Philosophical Society of Manchester. By William Henry, M.D. F.R.S. &c.\**

THE following tribute to the memory of the late President of the Literary and Philosophical Society has been drawn up, in compliance with a request, expressed to the writer from the chair, at an early meeting during the present session. It would, on some accounts, have been more satisfactory to him that the office should have fallen into other hands; but, conceiving a compliance with the requisition to be a duty, which he was not at liberty to decline, he has endeavoured to execute it with all the impartiality and fidelity in his power; and he trusts to the candour of the Society for that share of indulgence which he may reasonably claim in speaking of one to whom he was so nearly allied.

The late Mr. Henry was descended from a respectable family, which, for several generations, had resided in the county of Antrim. His paternal grandfather commanded a company of foot in the service of James II; and during the disturbed times, which, in Ireland, succeeded the revolution, was shot by an assassin in his own garden. The father of Mr. Henry, then an infant scarcely a year old, was taken under the generous protection of a neighbouring nobleman;† and, after being educated in Dublin at his lordship's expense, was brought over by him into Wales, when he had nearly attained the age of manhood.

\* Read before the Literary and Philosophical Society of Manchester.

† Viscount Bulkley.





an apothecary at Oxford. In this situation, he was treated by Mr. Malbon with the indulgence and confidence of a friend; and his time was chiefly spent in visiting patients of the higher class, a majority of whom were members of the University. Among the students at Oxford were several, who, recognizing Mr. Henry as a former associate, renewed their acquaintance with him, and afforded him the most friendly countenance. His leisure hours were, therefore, spent most agreeably and profitably in the different colleges; and his taste for literary pursuits was encouraged and confirmed. At Oxford, he had an opportunity of attending a course of anatomical lectures, in which the celebrated John Hunter, then a young man, was employed as demonstrator.

From Mr. Malbon, who was become affluent, Mr. Henry received a strong mark of esteem and confidence in the offer of a future partnership. To have accepted this, it would have been necessary that he should have qualified himself to matriculate, which would have required the completion of a residence of seven years. But other views in life, which were inconsistent with so long a season of expectation, induced him to decline the proposal; and in the year 1759, he settled at Knutsford, where he soon afterwards married. After remaining five years at this place, he embraced the opportunity of succeeding to the business of a respectable apothecary in Manchester; where he continued for nearly half a century to be employed in medical attendance, for the most part on the more opulent inhabitants of the town and neighbourhood.

Soon after Mr. Henry's settlement in Manchester, the late Dr. Percival removed to the same town from Warrington. That eminent physician was early inspired with the same ardent zeal for the cultivation of professional and general knowledge, which afterwards so much distinguished him. Congeniality of taste and pursuits led to a frequent intercourse between Dr. Percival and the subject of this memoir; and the moral qualities of each cemented their connexion into a friendship, which continued without interruption until it was terminated by the death of Dr. Percival, in 1804. It was about the same early period that he formed an acquaintance with that excellent man, and upright magistrate, the late Mr. Bayley, of Hope-Hall, and much of the happiness of his future life was owing to the mutual esteem and confidence, and to the frequent intercourse, which continued to exist between them for more than thirty years.\*

During his apprenticeship, Mr. Henry had manifested a decided taste for chemical pursuits, and had availed himself of all the means in his power, limited as indeed they were, to become experimentally acquainted with that science. This taste he continued to indulge after his settlement in life; and,

\* An interesting biographical sketch of Mr. Bayley, written by Dr. Percival, appeared in one of the volumes of the *Monthly Magazine* for the year 1802.

having made himself sufficiently master of what was ascertained in that department of knowledge, he felt an ambition to extend its boundaries. In the year 1771, he communicated to the Royal College of Physicians of London, "An improved Method of preparing Magnesia Alba," which was published in the second volume of their Transactions. Two years afterwards it was reprinted, along with essays on other subjects, in a separate volume, which was dedicated by Mr. Henry to his friend Dr. Percival.

The calcination of magnesia had, at that time, been practised only in connexion with philosophical inquiries. Dr. Black, in an essay, which is still perhaps not surpassed in chemical philosophy, as a beautiful example of inductive reasoning, had fully established the differences between magnesia in its common and in its calcined state; but he does not appear to have made trial of the pure earth as a medicine, though several inconveniences, from its use in the common form, had long before been pointed out by Hoffman.\* On this subject, Mr. Henry's claims extend to the free disclosure of his improvements; to the early and strenuous recommendation of the medicinal use of pure magnesia; and to the discovery of some of its chemical agencies. It is but justice to him to state that his recommendation of its employment as a medicine was perfectly disinterested; for it was not till his work was printed, and on the eve of issuing from the press, that the preparation of magnesia for sale was suggested to him by a friend, in a letter relating to the intended publication, which is still preserved as a part of his correspondence. Before carrying this suggestion into effect, he thought it proper to consult Sir John Pringle, Sir Clifton Wintringham, Dr. Warren, and some other leading members of the College of Physicians, as to their opinion of the propriety of the measure; and he did not adopt it until those gentlemen had declared it to be not more adviseable on his own account than on that of the public.

Soon after the publication of the small volume of Essays, Mr. Henry found himself involved in a controversy, arising out of some remarks in the appendix, respecting which, as the subject was of temporary interest, it is unnecessary to enter into particulars. It is sufficient to state that the accuracy of some of his experiments, which had been called in question, was confirmed by the concurrent testimony of Dr. Percival and Dr. Aikin; and that the chemical properties, first ascertained by him to belong to pure magnesia, were considered by Bergman and by Macquer as worthy of being incorporated into their respective histories of that earth.

It was probably in consequence of the publication of these Essays, that Mr. Henry was admitted into the Royal Society of

\* Hoffman. Oper. tom. iv. p. 381.

London, of which he became a Fellow in May, 1775. The persons most active in promoting his election were Sir John Pringle and Dr. Priestley; and he had the advantage not only of the vote, but of the favourable influence of Dr. Franklin, who happened at that time to be in London. Several years afterwards, the same venerable philosopher, when in the 81st year of his age, presided at the meeting of the American Philosophical Society, at which Mr. Henry was elected a member, and again honoured him with his suffrage.\*

The writings of the celebrated Lavoisier were introduced by Mr. Henry to the notice of the English reader in 1776. The earliest work of that philosopher was a volume, consisting partly of an historical view of the progress of pneumatic chemistry from the time of Van Helmont downwards; and partly of a series of original essays, which are valuable as containing the germs of his future discoveries. To this work, Mr. Henry added, in the notes, occasional views of the labours of contemporary English chemists. A few years afterwards he translated, and collected into a small volume, a series of *Memoirs*, communicated by M. Lavoisier to the Paris Academy of Sciences, when the views of that philosopher, respecting the anti-phlogistic theory of chemistry, were more fully unfolded. In undertaking the translation of these works, he was influenced by a desire to place within the reach of English readers, among whom the knowledge of the French language was then confined to comparatively few, the pleasure and conviction which he had himself derived from those admirable models of philosophical inquiry.

Notwithstanding the large share of professional employment to which Mr. Henry had now attained, he still continued to engage frequently in experimental pursuits, the results of which, at this time, were communicated to the world chiefly through the publications of his friends Dr. Priestley and Dr. Percival. Of these, the most important were some *Experiments on the Influence of Fixed Air on Vegetation*, by which he endeavoured to show, that though fixed air is injurious, when unmixed, to the vegetation of plants, yet that when mingled in small proportion with common air, it is favourable to their growth and vigour. The facts established by this inquiry were communicated to Dr. Priestley; and it is creditable to the candour of that distinguished philosopher, that he was anxious to make them public, not only for their general merit, but because, in one or two points, the results disagreed with his own. "I am much pleased," Dr. Priestley replies, "with the experiments mentioned in your letter, and if you have no objection, shall be glad to insert the greatest part of it in my Appendix, which I am just sending to the printer's. I the rather wish it, as a few of the experiments terminate differently from those that I shall publish,

\* This circumstance is stated in a letter from Dr. Rush to Mr. Henry, dated Philadelphia, July 29, 1786.



anxious desire to fulfil his duties as a member of it. To him, on its being first regularly organized in the winter of 1781, was confided the office of one of the secretaries. At a subsequent period, he was advanced to the station of Vice-President, and in the year 1807, on the vacancy occasioned by the death of the Rev. George Walker, F.R.S., he received from the Society, and retained during the rest of his life, the highest dignity which it has to bestow.

The "Memoirs of Albert de Haller," which were published by Mr. Henry in 1783, and dedicated to this Society, were derived partly from a French Eloge, and partly from information communicated by the late Dr. Foart Simmons. A more complete view of the life and acquirements of that extraordinary man might have been collected, at a subsequent period, from other publications of the same kind, which were addressed to different learned societies on the continent. In one respect, Mr. Henry appears to have taken too favourable a view of the character of Haller, in ascribing to him gentleness of disposition; for that illustrious, and in the main excellent, person, seems to have been a man of quick passions, and not sufficiently reserved in the expression of them; as may be gathered from his controversy with Dr. Whytt, of Edinburgh. Haller is represented also by his biographer as afflicted with the personal defect of weak eyes; which, from a passage in his *Physiology*,\* appears not to have been correct. "*Aquæ puræ*," he says, "*qua ab anno ætatis 18 sola utor, tribuo, quod post tot in fulgido sole susceptos microscopicos labores, omnibus sensibus, et oculis potissimum, non minus valeam, quam puer valui.*"

During the long season of Mr. Henry's activity as a member of this Institution, his communications to it were very frequent. Many of these were intended only to excite an evening's discussion, and, having served that purpose, were withdrawn by their author; but the number is still considerable, which are preserved in the Society's published volumes. As might be expected, they are of various degrees of merit, but there are among them two papers, which have contributed greatly to his reputation as a chemical philosopher.†

\* Tom. vi. p. 240, edit. 2. Lausannæ.

† The following is a list of Mr. Henry's papers that are dispersed through the printed Memoirs of this Society.

In vol. i. (1.) An Essay on the Advantages of Literature and Philosophy in general, and especially on the Consistency of Literary and Philosophical with Commercial Pursuits.

(2.) On the Preservation of Sea Water from Putrefaction by Means of Quicklime.

(3.) On the Natural History and Origin of Magnesian Earth, particularly as connected with those of Sea Salt and Nitre, with Observations on some Chemical Properties of that Earth, which have been hitherto unknown or undetermined.

In vol. ii. (1.) Experiments on Ferments and Fermentation, by which a Mode of exciting Fermentation in Malt Liquors, without the Aid of Yeast, is pointed out; with an Attempt to form a new Theory of that Process.





stance to be dyed, prevents it from exerting its attraction for colouring matter. The ultimate object of these preliminary steps, he states to be the obtaining a white ground that may enable the colours to display the full brilliancy of their several tints. To explain the preparation of cotton for the Turkey-red dye, he endeavours to prove that cotton requires for this purpose to be approximated in composition to the nature of an animal substance. He next offers a classification of the *Materia Tinctoria*, and some general speculations on the nature of colouring matter.

In the second part of the essay, Mr. Henry investigates the mode of action of those substances, which, though themselves destitute of colour, are important agents in the processes of dyeing. Substances of this kind had received, from the French dyers, the name of *mordants*, because it was imagined that they corroded and removed something which mechanically opposed the entrance of the colouring matter into the pores of the material to be dyed. To destroy this erroneous association, Mr. Henry proposes that the word *basis* should be substituted, as a general term, to denote every substance which, having an affinity both for the colouring matter and for the material to be dyed, is capable of serving as an intermedium between the two; and that a specific epithet should be added, to distinguish each particular variety. In this essay, Mr. Henry, for the first time, explained the true nature of the liquor which is employed for affording the aluminous basis, prepared by mixing the solutions of alum and of sugar of lead. This liquor he showed to be essentially a compound of pure clay or alumine with acetic acid; and its superiority over a solution of common alum, for yielding the earthy base in dyeing, he ascribes partly to the less affinity of the acetic acid than of the sulphuric for alumine, and partly to the greater volatility of the acetic acid, when exposed to a moderate increase of temperature. The remainder of the paper is chiefly occupied with the details of the operations then practised for dyeing Turkey-red; with a theory of the process; and with a general view of the mode of action of the individual mordants or bases. The method of dyeing Turkey-red has been since much improved and simplified, though its theory is, even yet, far from being well understood. But the opinions, first inculcated by Mr. Henry, respecting the action of mordants, evince a remarkable superiority to the prejudices, with which he found the subject encumbered, and are, indeed, those which are still held by the latest and best writers on the principles and practice of dyeing.

In the year 1783, an institution arose out of this Society, which had great merit, not only in its plan and objects, but in the ability exerted by the several persons who were concerned in their fulfilment. It was destined to occupy in a rational and instructive manner the evening leisure of young men, whose time during the day was devoted to commercial employments.



investigation of general principles, and habituated to the indulgence of enlightened and comprehensive views, is compelled, as Lord Bacon expresses it, "to contract the sight of his mind as well as to dilate it." It is, therefore, not unreasonable to expect that this intellectual habit may, in other persons, be reversed; and that he whose attention is for the most part given to employments demanding no powers beyond those of patient industry, may occasionally, without detriment to his temporal concerns, take a wider range, and elevate his mind to the perception of literary pleasures, or of the general truths of moral, intellectual, or natural science.

It must, however, be acknowledged, that there is considerable danger, lest objects which ought to be held by young men devoted to active life as only of secondary importance should acquire an undue share of their estimation, and inspire a disrelish for more necessary but less attractive occupations. This alloy to the advantages of knowledge may, it appears to me, be avoided by carefully selecting such studies as may not be inconsistent with the business of after life, and by pursuing them only to a prudent and temperate extent, estimating them indeed as we do those lighter ornaments of a building, which are of no value, excepting as they add grace and beauty to substantial and durable forms. More especially it seems to be important, as a safeguard against this apprehended abuse of learning, that there should early be mingled with its pursuit a due share of those employments which in future are to constitute the main business of life; and that those habits, both intellectual and moral, should be early and assiduously fostered, which are essential to success in future commerce with the world, and to the acquirement of a moderate portion of its advantages. This rational and prudent alliance between the avocations that support and those that embellish and give zest to life was the utmost that was ever contemplated by the founders of an institution, which certainly deserved better success; and to which other establishments, since formed with the same views, but under happier auspices, in the metropolis and some commercial towns, bear, in their leading features, a striking resemblance.

Besides the lectures on the general principles of chemistry, Mr. Henry delivered a course on the arts of bleaching, dyeing, and calico-printing; and to render this course more extensively useful, the terms of access to it were made easy to the superior class of operative artizans. It was at this period that the practical application was made in France of a philosophical discovery to one of the arts which Mr. Henry was engaged in teaching, that shortened, by several weeks, the duration of its processes. In 1774, Scheele, a Swedish chemist, distinguished by the number and great importance of his contributions to chemical science, in the course of some experiments on manganese, discovered the substance known successively by the names of





though he did not embark in new experimental inquiries, yet he continued for many years to feel a warm interest in the advancement of science; and to maintain an occasional correspondence with persons highly eminent for their rank as philosophers, both in this and other countries.\* His medical avocations had greatly increased, and for a further interval of 15 or 20 years, he had a share of professional employment which occupied by much the greater portion of his time. This, and the superintendence of some chemical concerns, prevented him from attempting more than to keep pace with the progress of knowledge. He was in no haste, however, to claim that exemption from active labour to which advanced age is fairly entitled; and it was not till a very few years before his death that he retired from the exercise of the medical profession.

The summers of the years 1814 and 1815 were spent by Mr. Henry in the country, a mode of life, which, now that his season of active exertion was passed, was peculiarly suited to him, not only by the tranquil retirement which it afforded, but by its enabling him to indulge that sensibility to the charms of rural scenery, which, perhaps, can exist in perfection only in a pure and virtuous mind. His perception of these pleasures was at no period more lively than after he had entered his 81st year. In a note addressed to the writer of these pages, in the autumn of 1815, he describes, in animated language, one of those events which so agreeably diversify the face of nature in the country. "Yesterday," he says, "we had one of the most beautiful appearances in the garden I ever witnessed. Every leaf, every petal, every projecting fibre, was beset with a minute globule of water; and when the sun shone upon the flowers and shrubs, they seemed as if studded with myriads of brilliants. The gossamer too with which the hedges were covered was adorned with the same splendid appendages. The cause," he adds, "of this deposition of moisture must, I suppose, have been electrical."

The winter of the year 1815, which Mr. Henry passed in Manchester, was a season of greater suffering than was usual to him; for though of a delicate constitution, yet he happily, even at this advanced time of life, enjoyed an almost entire exemption from painful diseases. During this winter, he was much distressed by cough and difficult breathing, and his bodily strength rapidly declined. In the spring of the following year, he returned into the country, but not to the enjoyments which he had before derived from it. He was unable to take his

\* A considerable collection of letters to Mr. Henry from persons of this description has been preserved; but the subjects of them have for the most part been long ago brought before the public by their respective writers. The letters are, therefore, chiefly valuable to the family of the deceased, as unequivocal proofs of the respect and esteem felt towards him by those who were best qualified to judge of his merits. Many of them are from learned foreigners, with whom he had enjoyed opportunities of personal intercourse during their visits to Manchester.



native strength of his memory, unassisted by any artificial arrangement, a knowledge of history, remarkable for its extent and precision; and he was always eager to discuss those questions of general policy which are to be decided partly by an appeal to historical evidence, and partly by a consideration of the nature of man, and of his claims and duties as a member of society. No representation of him would indeed be complete that failed to notice the animation with which he entered into arguments of this kind, or the zeal and constancy with which he defended his political opinions—opinions which, in him, were perfectly disinterested and sincere, but which perhaps disposed him to allow more than its due weight to the aristocratical part of our mixed government. It would be unjust to him, however, not to state, that no man could more cordially disapprove, or more unreservedly condemn, every unwarrantable exertion of power; or could more fervently desire the extension of the blessings of temperate freedom to all mankind. It was this feeling that led him to use his strenuous exertions as a member of one of the earliest societies for procuring the abolition of the African Slave Trade; and when that great object was at length accomplished, he was affected with the most lively joy and gratitude on the downfall of a traffic which had long been a disgraceful stain on our national character.

Of his moral excellencies there can be no inducement to offer an overcharged picture to a Society by many of whose surviving members he was intimately known and justly appreciated. Foremost among the qualities of his heart was a warmth of generous emotion which evinced itself in an enthusiastic admiration of virtue; in an indignant disdain and unqualified reprobation of vice, oppression, or meanness; and in the prompt and unrestrained exercise of the social affections. In temper, he was frank, confiding, and capable of strong and lasting attachments; quick, it must be acknowledged, in his resentments; but remarkably placable and anxious whenever he thought he had inflicted a wound to heal it by redoubled kindness. No man could be more free from all stain of selfishness; more moderate in his desire of worldly success; or more under the influence of habitual contentment. This was in a great measure the result of his having early weighed the comparative value of the different objects of life, and of his steady and consistent pursuit of knowledge and virtue, as the primary ends of an intelligent being.

In very advanced age, though his body was enfeebled, his mind retained much of that wholesome elasticity and vigour which always belonged to it. He was still enabled, by the almost perfect preservation of his sight, to spend a great portion of every day in reading; but at this period he derived greater pleasure from works of literature than from those of science, and especially from his favourite study of history. During the winter immediately preceding his death, beside several standard historical works, he read with avidity one which had been recently



The grounds of this conviction and some recent experiments confirming it are stated in the following paper.

It is well known that heat is liberated by voltaic apparatus in a manner and degree which has not been imitated by means of mechanical electricity; and that the latter, while it strikes at a greater distance, and pervades conductors with much greater speed, can with difficulty be made to effect the slightest decompositions. Wollaston, it is true, decomposed water by means of it; but the experiment was performed of necessity on a scale too minute to permit of his ascertaining whether there were any divellent polar attractions exercised towards the atoms, as in the case of the pile. The result was probably caused by mechanical concussion, or that process by which the particles of matter are dispersed when a battery is discharged through them. The opinion of Dr. Thomson, that the fluid of the pile is in quantity greater, in intensity less, than that evolved by the machine, is very inconsistent with the experiments of the chemist above-mentioned, who, before he could effect the separation of the elements of water by mechanical electricity was obliged to confine its emission to a point imperceptible to the naked eye. If already so highly intense, wherefore the necessity of a further concentration? Besides, were the distinction made by Dr. Thomson correct, the more concentrated fluid generated by a galvanic apparatus of a great many small pairs, ought most to resemble that of the ordinary electricity; but the opposite is the case. The ignition produced by a few large galvanic plates, where the intensity is of course low, is a result most analogous to the chemical effects of a common electrical battery. According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air, with immeasurable celerity, and distributing itself in the interior of bodies, communicates a reciprocally repellent power to atoms but not to masses. Electricity does not radiate in or through any matter; and while it pervades some bodies, as metals, with almost infinite velocity; by others, it is so far from being conducted that it can only pass through them by a fracture or perforation. Distributing itself over surfaces only, it causes repulsion between masses, but not between the particles of the same mass. The disposition of the last mentioned principle to get off by neighbouring conductors, and of the other to combine with the adjoining matter, or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification of their properties consequent to some such union, they could not, in piles of thousands of pairs, be carried forward through the open air and moisture; the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric.







nescent ignition, was too minute to allow him to make the observations which I had in view.

Twenty copper and twenty zinc plates, about 19 inches square, were supported vertically in a frame, the different metals alternating at one half inch distance from each other. All the plates of the same kind of metal were soldered to a common slip, so that each set of homogeneous plates formed one continuous metallic superficies. When the copper and zinc surfaces thus formed are united by an intervening wire, and the whole immersed in an acid, or aceto-saline solution, in a vessel devoid of partitions, the wire becomes intensely ignited; and when hydrogen is liberated, it usually takes fire, producing a very beautiful undulating or corruscating flame.

I am confident, that if Volta and the other investigators of galvanism, instead of multiplying the pairs of galvanic plates, had sought to increase the effect by enlarging one pair as I have done (for I consider the copper and zinc surfaces as reduced to two by the connexion), the apparatus would have been considered as presenting a new mode of evolving heat as a primary effect independently of electrical influence. There is no other indication of electricity when wires from the two surfaces touch the tongue than a slight taste, such as is excited by small pieces of zinc and silver laid on it and under it, and brought into contact with each other.

It was with a view of examining the effects of the proximity and alternation in the heterogeneous plates that I had them cut into separate squares. By having them thus divided, I have been enabled to ascertain that when all of one kind of metal are ranged on one side of the frame, and all of the other kind on the other side of it, the effect is no greater than might be expected from one pair of plates.

Volta, considering the changes consequent to his contrivance as the effect of a movement in the electric fluid, called the process electro-motion, and the plates producing it electro-motors. But the phenomena show that the plates, as I have arranged them, are *calori-motors*, or heat movers, and the effect *calori-motion*. That this is a new view of the subject, may be inferred from the following passage in Davy's *Elements*. That great chemist observes, "When very small conducting surfaces are used for conveying very large quantities of electricity, they become ignited; and of the different conductors that have been compared, charcoal is most easily heated by electrical discharges,\* next iron, platina, gold, then copper, and lastly zinc. The phenomena of electrical ignition, whether taking place in gaseous, fluid, or solid bodies, always seem to be the result of a violent exertion of the electrical, attractive, and repellent powers, which may be connected with motions of the

\* The conclusions are drawn from experiments made by the electricity of the voltaic apparatus.

particles of the substances affected. That no subtile fluid, such as the matter of heat has been imagined to be, can be discharged from these substances, in consequence of the effect of the electricity, seems probable, from the circumstance that a wire of platina may be preserved in a state of intense ignition in vacuo, by means of the voltaic apparatus, for an unlimited time; and such a wire cannot be supposed to contain an inexhaustible quantity of subtile matter."

But I demand where are the repellant and attractive powers to which the ignition produced by the calorimotor can be attributed? Besides, I would beg leave respectfully to inquire of this illustrious author, whence the necessity of considering the heat evolved under the circumstances alluded to as the effect of the electrical fluid; or why we may not as well suppose the latter to be excited by the heat? It is evident, as he observes, that a wire cannot be supposed to contain an inexhaustible supply of matter however subtile; but wherefore may not one kind of subtile matter be supplied to it from the apparatus as well as another? Especially, when to suppose such a supply is quite as inconsistent with the characteristics of pure electricity as with those of pure caloric.

It is evident from Mr. Children's paper, in the *Annals of Philosophy*, on the subject of his large apparatus, that the ignition produced by it was ascribed to electrical excitement.

For the purpose of ascertaining the necessity of the alternation and proximity of the copper and zinc plates, it has been mentioned that distinct square sheets were employed. The experiments have since been repeated and found to succeed by Dr. Patterson and Mr. Lukens, by means of two continuous sheets, one of zinc, the other of copper, wound into two concentric coils or spirals. This, though the circumstance was not known to them, was the form I had myself proposed to adopt, and had suggested as a convenient for a galvanic apparatus to several friends at the beginning of the winter;\* though the consideration above stated induced me to prefer for a first experiment a more manageable arrangement.

Since writing the above, I find that when, in the apparatus of 20 copper and 20 zinc plates, 10 copper plates on one side are connected with 10 zinc on the other, and a communication made between the remaining 20 by a piece of iron wire, about the eighth of an inch in diameter, the wire enters into a vivid state of combustion on the immersion of the plates. Platina wire equal to No. 18 (the largest I had at hand) is rapidly fused if substituted for the iron.

This arrangement is equivalent to a battery of two large galvanic pairs, excepting that there is no insulation, all the plates

\* Especially to Dr. T. P. Jones and Mr. Rubens Peale, who remember the suggestion.

being plunged in one vessel. I have usually separated the pairs by a board, extending across the frame merely.

Indeed when the 40 plates were successively associated in pairs of copper and zinc, though suspended in a fluid held in a common recipient without partitions, there was considerable intensity of galvanic action. This shows that, independently of any power of conducting electricity, there is some movement in the solvent fluid which tends to carry forward the galvanic principle from the copper to the zinc end of the series. I infer that electro-caloric is communicated in this case by circulation, and that in non-elastic fluids the same difficulty exists as to its retrocession from the positive to the negative end of the series, as is found in the downward passage of caloric through them.

It ought to be mentioned, that the connecting wire should be placed between the heterogeneous surfaces before their immersion, as the most intense ignition takes place immediately afterwards. If the connexion be made after the plates are immersed, the effect is much less powerful; and sometimes after two or three immersions, the apparatus loses its power, though the action of the solvent should become in the interim much more violent. Without any change in the latter, after the plates have been for some time suspended in the air, they regain their efficacy. I had observed in a galvanic pile of 300 pairs of two inches square a like consequence resulting from a simultaneous immersion of the whole. (See Plate XCVI, fig. 3.) The bars holding the plates were balanced by weights, as window sashes are, so that all the plates could be very quickly dipped. A platina wire, No. 18, was fused into a globule, while the evolution of potassium was demonstrated by a rose-coloured flame arising from some potash which had been placed between the poles. The heat, however, diminished in a few seconds, though the greater extrication of hydrogen from the plates indicated a more intense chemical action.

Agreeably to an observation of Dr. Paterson, electrical excitement may be detected in the apparatus by the condensing electroscope, but this is no more than what Volta observed to be the consequence of the contact of heterogeneous metals.

The thinnest piece of charcoal intercepts the calorific agent, whatever it may be. In order to ascertain this, the inside of a hollow brass cylinder, having the internal diameter two inches, and the outside of another smaller cylinder of the same substance, were made conical and correspondent, so that the greater would contain the less, and leave an interstice of about one-sixteenth of an inch between them. This interstice was filled with wood, by plugging the larger cylinder with this material, and excavating the plug till it would permit the smaller brass cylinder to be driven in. The excavation and the fitting of the cylinders was performed accurately by means of a turning

lathe. The wood in the interstice was then charred by exposing the whole covered by sand in a crucible to a red heat. The charcoal, notwithstanding the shrinkage consequent to the fire, was brought into complete contact with the inclosing metallic surfaces by pressing the interior cylinder further into the exterior one.

Thus prepared, the exterior cylinder being made to touch one of the galvanic surfaces, and a wire brought from the other galvanic surface into contact with the outside cylinder, was not affected in the least, though the slightest touch of the interior one caused ignition. The contact of the charcoal with the containing metals probably took place throughout a surface of four square inches, and the wire was not much more than the hundredth part of an inch thick; so that unless it were to conduct electricity about 40,000 times better than the charcoal, it ought to have been heated, if the calorific influence of this apparatus result from electrical excitement.

I am led finally to suppose that the contact of dissimilar metals, when subjected to the action of solvents, causes a movement in caloric as well as in the electric fluid, and that the phenomena of galvanism, the unlimited evolution of heat by friction, the extrication of gaseous matter without the production of cold, might all be explained by supposing a combination between the fluids of heat and electricity. We find scarcely any two kinds of ponderable matter which do not exercise more or less affinity towards each other. Moreover, imponderable particles are supposed highly attractive of ponderable ones. Why then should we not infer the existence of similar affinities between imponderable particles reciprocally? That a peculiar combination between heat and light exists in the solar beams is evident from their not imparting warmth to a lens through which they may pass, as do those of our culinary fires.

Under this view of the case, the action of the poles in galvanic decomposition is one of complex affinity. The particles of compounds are attracted to the different wires agreeably to their susceptibilities to the positive and negative attraction, and the caloric leaving the electric fluid with which it had been combined unites with them at the moment that their electric state is neutralized.

As an exciting fluid, I have usually employed a solution of one part sulphuric acid and two parts muriate of soda, with 70 of water; but, to my surprise, I have produced nearly a white heat by an alkaline solution barely sensible to the taste.

For the display of the heat effects, the addition of manganese, red lead, or the nitrates, is advantageous.

The rationale is obvious. The oxygen of these substances prevents the liberation of the gaseous hydrogen, which would carry off the caloric; adding to diluted muriatic acid, while act-



ing on zinc, enough red lead to prevent effervescence, the temperature rose from 70 to 110 Fahr.

The power of the calorimotor is much increased by having the communication between the different sheets formed by very large strips or masses of metal. Observing this, I rendered the sheets of copper shorter by half an inch for a distance of four inches of their edges, where the communication was to be made between the zinc sheets; and, *vice versa*, the zinc was made in the same way shorter than the copper sheets where these were to communicate with each other. The edges of the shortened sheets being defended by strips of wood, tin was cast on the intermediate protruding edges of the longer ones, so as to embrace a portion of each equal to about one quarter of an inch by four inches. On one side, the tin was made to run completely across, connecting at the same time 10 copper and 10 zinc sheets. On the other side, there was an interstice of above a quarter of an inch left between the stratum of tin embracing the copper and that embracing the zinc plates. On each of the approaching terminations of the connecting tin strata was soldered a kind of forceps, formed of a bent piece of sheet brass, furnished with a screw for pressing the jaws together. The distance between the different forceps was about two inches. The advantage of a very close contact was made very evident by the action of the screws; the relaxation or increase of pressure on the connecting wire by turning them being productive of a correspondent change in the intensity of ignition.

It now remains to state, that by means of iron ignited in this apparatus, a fixed alkali may be decomposed extemporaneously. If a connecting iron wire, while in combustion, be touched by the hydrate of potash, the evolution of potassium is demonstrated by a rose-coloured flame. The alkali may be applied to the wire in small pieces in a flat hook of sheet iron. But the best mode of application is by means of a tray made by doubling a slip of sheet iron at the ends, and leaving a receptacle in the centre, in which the potash may be placed covered with filings. This tray being substituted for the connecting wire, as soon as the immersion of the apparatus causes the metal to burn, the rose-coloured flame appears; and if the residuum left in the sheet iron be afterwards thrown into water, an effervescence sometimes ensues.

I have ascertained that an iron heated to combustion by a blacksmith's forge fire will cause the decomposition of the hydrate of potash.

The dimensions of the calorimotor may be much reduced without proportionably diminishing the effect. I have one of 60 plates within a cubic foot, which burns off No. 16, iron wire. A good workman could get 120 plates of a foot square within a hollow cube of a size no larger. But the inflammation of the



## ARTICLE III.

*On the Construction of Sails.* By Col. Beaufoy, F.R.S.  
(With a Plate.)

(To Dr. Thomson.)

MY DEAR SIR,

Bushey Heath, July 20, 1819.

SHOULD you think the accompanying paper on the Construction of Sails worthy a place in the *Annals of Philosophy*, you will much oblige me by its insertion.

I remain, my dear Sir,

Very sincerely yours,

MARK BEAUFOY.

The generality of vessels being impelled by wind, it is a most important circumstance in naval mechanics that sails be so made as to produce the greatest effect; for should they be improperly constructed, the ship-builder will, to little purpose, have exerted his skill in giving the hull of the vessel the most advantageous form for dividing the water. The time most important that sails should be scientifically made is when beating to windward, especially off a lee shore, as the safety of the vessel and cargo, and, what is of much more consequence, the lives of the crew, may entirely depend on their action.

It is not my intention to write a treatise on sail making, or to describe their variety of shapes; I shall, therefore, confine my remarks to those sails technically termed fore and aft sails, and the form in which I conceive they ought to be made to enable them to expose a flat surface to the wind, and which I have practised with success.

When sails are to be made, the first operation is to cut cloth by cloth, the width being regulated by the length of the yard, boom, gaff, or stay, and the depth by the height of the mast; the most simple mode of forming the sail is by sewing the selvages of the canvass together, and making the seams rectilineal, as represented in Pl. XCVI, fig. 4; but as sails when large, and constructed of canvass alone, are not sufficiently strong to resist the impulse of a powerful wind, the edges are usually surrounded with cordage, called bolt-ropes, to diminish the strain, and prevent renting. When sewing on the bolt-rope, it is necessary to gather up at the head and fore leech about one-seventh of the canvass, by which means in hoisting and spreading the head of the sail, the stress is transferred from the canvass to the cordage; \* but a sail thus made will not, when set, expose a plane surface to the wind; for, besides being concave, some of the cloths in the after-leech will have the effect of back sails, which impedes the vessel's progressive velocity, and materially augments the leeway. To remedy this two-fold disadvantage,

\* In the fore and after-leech, there should be the same length of bolt-rope as canvass, in other words, no part of the canvass should be gathered in.



it is proposed to give the sails a circular shape, as represented in fig. 5, which may be done in the following manner: The hoist of the sail, A B (fig. 4), the spread of the head, B C, the length of the after-leech, C D,\* the dimensions of the curved part (called the roach), D E, and the versed sine, A Z (fig. 5), being fixed, proceed to calculate the radius of a circle that will pass through the three points, C Z D, by dividing the square of half the chord by the versed sine, and to the quotient adding the versed sine, and half the sum is the radius. In this case, A Z is one foot, C D 22 feet 1 inch, or 22·0833; the half is 11·0416; the square 121·93; to the number add 1; and as an unit neither divides nor multiplies, half the sum, 61·465 feet, is the radius. On the radius from Z towards p, set off in succession the width of the canvass of which the sail is to be made, deducting the breadth of the seam, and through these points draw the requisite number of concentric circles, intersecting the roach, A D, gaff, C B, and mast, B A, and the length of the different gores will be obtained by measurement. The distance between the head and foot of the sail must next be ascertained in those places where the concentric circles cross it, which distances are the chords of the different arcs. With these chords and the radii calculate the lengths of the respective arcs, and the differences between the lengths of the chords and the lengths of the arcs is the quantity of canvass to be gathered up in the inner edge when sewing it to the next cloth; and similar operations are to be performed until the sail be completed. The following table may be found useful in illustrating my meaning.

Cloths.	Radil.	Circumfer.	Chords.		Angles.	Length of Arcs.		Diff.
	Feet.	Feet.	Feet.	In.		Feet.	In.	
1	61·465	386·20	22	1	20° 41' 10"	22	2·27	1·27
2	60·450	379·82	21	8½	20 41 10	21	9·80	1·30
3	59·435	373·44	21	3	20 35 10	21	4·21	1·21
4	58·420	367·06	20	11	20 37 30	21	0·34	1·24
5	57·405	360·68	20	4½	20 27 10	20	5·88	1·13
6	56·390	354·30	19	10	20 15 30	19	11·16	1·16
7	55·375	347·92	19	3	20 01 10	19	4·15	1·15
8	54·360	341·54	18	8	19 46 30	18	9·02	1·02
9	53·345	335·16	18	2	19 36 30	18	3·00	1·00
10	52·330	328·78	17	6	19 15 10	17	6·96	0·96
11	51·315	322·40	16	10	18 52 30	16	10·74	0·74
12	50·300	316·02	16	2	18 29 10	16	2·79	0·79
13	49·285	309·64	15	7	18 11 30	15	7·68	0·68
14	48·270	303·26	13	10	16 28 30	13	10·45	0·45
15	47·255	296·88	12	10	15 36 30	12	10·38	0·38
16	46·240	290·50	10	1	12 31 10	10	1·20	0·20
17	45·225	284·12	8	1	10 15 10	10	1·07	0·07
18	44·210	277·74	6	0	7 47 00	6	0·06	0·06
19	43·195	271·36	3	9	4 58 30	3	9·00	0·00
20	42·180	264·98	1	7	2 08 30	1	7·00	0·00
1	2	3	4		5	6	7	

\* The mainsail when hoisted appears to most advantage when the after-leech is sufficiently long to permit the gaff to be parallel to the fore-part of the jib.

Column 1 contains the number of cloths which compose the sail; 2. The radii of the concentric circles; 3. The circumferences of the circles; 4. The length of the chords of the circular segment; 5. The angles subtended by the chords; 6. The length of the arcs; and 7. The differences between the length of each chord, and its respective arc, which is the quantity of canvass to be gathered in at the seams. Column 2 is formed by continually subtracting the width of the canvass 1·015 from the radius of the after-leech, 61·465; and column 3, by deducting the constant quantity, 6·38, the diminution of the peripheries. Such were the construction of the sails of my land-sailing machine,\* which, in an open and extensive meadow, with a moderate breeze, and five points from the wind, required a gallop to keep pace with it.

The versed sine of the mainsail is  $10^{\circ} 21'$ , but in sails made with canvass, No. 1 or No. 2, it is probable that the versed sine might be increased to 12 degrees without danger of the after-leech flapping; for the larger the versed sine, the better the sail will set, provided it be not so great as to produce the above-mentioned disadvantage.

The length of the chord and versed sine in degrees being given, the length of the versed sine and radius may be calculated from a table of natural sines, cosines, and versed sines, as follows: Suppose the chord of the after-leech to be 70 feet, and the versed sine 12 degrees, the natural versed sine of 12 degrees is ·021852, the natural cosine ·97815, the natural sine ·20791, radius being unity:  $\cdot 20791 : 35 \left( \frac{70}{2} \right) :: \cdot 021852 : 3 \cdot 6786 = 3$  feet 8·14 in. the versed sine;  $20791 : 35 :: 1 \cdot 97815 : 333 \cdot 01$ , then  $\frac{333 \cdot 01 + 3 \cdot 68}{2}$  is 168·34 feet the radius. To describe in the usual

manner a circular segment whose semidiameter amounts to 168 feet, in confined places is impossible: some other means must, therefore, be adopted, and it may be performed two ways: first, mechanically; and secondly, by finding a number of points and uniting them together by means of a flexible piece of wood, for which an eligible rule is given in Mr. Mungo Murray's book on naval architecture. The former method is frequently used by carpenters, joiners, and other artizans, and is as follows: An isosceles triangle is made of wood, whose base contains the number of feet and inches as the chord of the arc to be described, and whose altitude is equal to the versed sine of the arc; three pins are then put on the board, or paper, on which the segment is to be traced, one pin at each end of the chord, the third on the extremity of the radius; a pencil is next applied to the vertex of the triangle, and the whole made to slide on the three pins, and the segment is traced. It is evident that it would be an

\* The quantity of canvass in the three sails measured 312 superficial feet.



improvement if a machine was contrived to which could be adapted a variety of segments; and such an object, I conceive, might be obtained by having the oblique sides made of light materials, and united by a hinge, and a brace furnished with a screw and clamp being placed transversely, one end turning on a centre in one leg, and the other extremity sliding in the opposite leg. The triangle might be opened or shut the requisite distance, and fastened.

Stay-sails designed to be carried on a wind ought not to be cut with a square tack, the stay being a fixture on which the sails turn. One with a square tack is quadrilateral; it is, therefore, evident, if the materials of which the sail is composed be inflexible, a sail of this figure, when confined in three points, must remain parallel to the vessel's keel, and consequently produce no other effect than that of driving it bodily to leeward; nor will the custom of carrying the tack to windward render a sail of this form so efficacious in producing a progressive velocity as one of a triangular shape.

Formerly, it was usual to have a reef in the topsails to let out when running large, and it is reasonable to suppose some good purpose was to be answered in adopting a plan so inconvenient when suddenly obliged to haul on a wind, as the sail must be reefed before hoisted. Two circumstances, in my opinion, contribute to make a swelling sail, when running large, produce a greater effect than a flat one; first, because the wind is compressed in the hollow or concave part; and secondly, the convex part passes more readily through the stagnant air in front of the sail; and I am confirmed in this idea from rough experiments made with a concave cylinder, open at one end, and closed at the other. I found that when the open end was exposed to the wind, it was more resisted than when the other end received the shock. The annexed table shows the resistance of air in ounces avoirdupois, of a cylinder 13·541 inches in diameter and altitude, when exposed to the impulse in the direction of its axis, and likewise when placed in a vertical position.

Velocity in feet per second.	4	8	10	12	16	20
Position of the cylinder.	Horizontal	0·485	2·024	3·206	4·668	8·443
	Vertical...	0·352	1·126	1·631	2·213	3·576
Difference		0·133	0·898	1·575	2·455	4·869

Consequently, as the resistance of the cylinder sideways is so much less than that it encounters endways, it is evident that the bellying of the sail is advantageous to the sailing of the ship when before the wind.

In small sails, where strength is not of so much consequence, it would be preferable to have the cloths horizontal instead of vertical, as is plainly shown by ships' ensigns and flags in general;

the selvages of the bunting being always placed in the first position, which causes them to blow out without bagging.

It would be a two-fold improvement, if, in running large, the sheet of square sails was eased off; namely, first, the better division of the stagnant air in front; and secondly, the sails not being so violently stretched, they would stand better on a wind.

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#### ARTICLE IV.

##### *Researches on some important Points of the Theory of Heat.\**

By MM. Petit and Dulong. (Presented to the Academy of Sciences on April 12, 1819.)

THE considerations founded on the laws relative to the proportions of chemical compounds enable us to form respecting the constitution of bodies, ideas which, though arbitrarily established in several points, cannot, however, be regarded as absolutely vague and sterile. Convinced likewise that certain properties of matter would present themselves under more simple forms, and could be expressed by more regular and less complicated laws, if we could refer them to the elements upon which they immediately depend, we have endeavoured to introduce into the study of some of the properties which appear more intimately connected with the individual action of the material molecules, the most certain results of the atomic theory. The success which we have already met with makes us hope not only that this kind of consideration may contribute materially to the further progress of physics; but that the atomic theory itself will receive from it a new degree of probability, and will from it derive sure methods of determining the truth among different hypotheses all equally probable.

Among the properties of matter to which the considerations just mentioned are applicable, we shall choose, in the first place, as having more particularly fixed our attention, those which depend upon the action of heat. By directing our observations in a suitable manner, we have been led to discover simple relations between phenomena, the connexion of which had not been previously attended to; but the numerous points of view under which these phenomena may be examined, giving to our researches an extent which does not permit us to embrace the whole at one time, we have thought that it might be useful at present to make known the results to which we have come.

These first results relate to specific heats. The determination of this important element has been, as is well known, the object

\* Translated from the *Annales de Chimie et Physique*, x. 396.



The one which we have chosen appears to us to unite all the requisite conditions.

It is founded upon the laws of cooling. It is known that there exist between the velocity of cooling of different bodies placed in the same circumstances and the specific heats of the same bodies, relations, in consequence of which the ratio of the capacities may be deduced from that of the times of cooling. The first application of this principle was by Mayer, who satisfied himself that the capacities determined in this way differ little from those obtained for the same bodies by the method of mixture. Mr. Leslie, who has adopted the method of Mayer, has pointed out an additional precaution, of which the latter did not suspect the necessity; namely, to enclose the body on which we operate in an envelope, which must be always the same, in order to avoid the error which would result from any inequality in the radiating power of the surfaces. But the most important of all the causes of uncertainty, and to which neither Mayer nor Leslie paid any attention, is that which results from the unequal conductivity of the substances compared with each other. The influence of this cause is so much the less, the smaller the volume is of the bodies operated upon, and the slower the heat makes its escape from it. Our object then must be to fulfil these two conditions; but it is difficult to reconcile them, because when we diminish the mass of a body, we augment the velocity with which its heat is dissipated. However, by endeavouring to unite all the causes which contribute to retard the cooling of a given mass, we are enabled, as the experiments have shown, to place it in such circumstances that the difference in the conductivity of the substances operated on has no longer any sensible influence on the measure of the capacities.

The first method which presents itself for attaining that end is not to begin the observation till the temperature of the body is only a few degrees higher than that of the surrounding bodies. Accordingly all our experiments were made in an interval of temperature included between  $10^{\circ}$  and  $5^{\circ}$  centigrade of excess above the ambient medium. It is indispensable to measure the changes of temperature with the greatest possible care; for even a slight error in the estimation might occasion a great mistake in the result which it is the object to obtain. By operating, as we have said, at the same temperature for all the bodies, we avoid errors resulting from the graduation of the thermometer; and by observing this instrument through a glass, we can increase the size of its degrees so much as not to commit an error exceeding the 50th of a degree, which occasions a degree of uncertainty respecting the specific heat that may be overlooked. It is well known that all these precautions would be delusive if the temperature of the ambient medium were not rigorously the same in each case, and during the total duration of every experiment;



	Specific heats, that of water being 1.	Weight of the atoms, that of oxygen being 1.	Product of the weight of each atom by the cor- responding capa- city.
Bismuth. ....	0.0288	13.300	0.3830
Lead . ....	0.0293	12.950	0.3794
Gold . ....	0.0298	12.430	0.3704
Platinum ....	0.0314	11.160	0.3740
Tin. ....	0.0514	7.350	0.3779
Silver. ....	0.0557	6.750	0.3759
Zinc . ....	0.0927	4.030	0.3736
Tellurium ....	0.0912	4.030	0.3675
Copper . ....	0.0949	3.957	0.3755
Nickel . ....	0.1035	3.690	0.3819
Iron . ....	0.1100	3.392	0.3731
Cobalt . ....	0.1498	2.460	0.3685
Sulphur. ....	0.1880	2.011	0.3780

To make the law intelligible, which we propose to make known, we have joined, in the preceding table, to the specific heats of the different bodies, the relative weights of their atoms. These weights are deduced, as is known, from the ratios observed between the weights of the elementary substances that unite together. The care taken for some years in the determination of the proportions of most chemical compounds can only leave slight uncertainties with respect to the data which we have employed; but as no precise method exists of discovering the real number of atoms of each kind which enter into a combination, it is obvious that there must always be something arbitrary in the choice of the specific weight of the elementary molecules; but the uncertainty can be only in the choice of two or three numbers which have the most simple relation to each other. The reasons which have directed us in our choice will be sufficiently explained by what follows. We shall satisfy ourselves at present with saying, that there is none of the numbers on which we have fixed which does not agree with the best established chemical analogies.

We may now, in consequence of the data contained in the preceding table, calculate easily the ratio which exists between the capacity of atoms of a different kind. We may remark, that in order to pass from the specific heats furnished by the observations to those of the particles themselves, it is sufficient to divide the former by the number of particles contained in the same weight of the substances which we compare; but it is clear that the number of particles for equal weights of matter are reciprocally proportional to the density of the atoms. We shall obtain, therefore, the result wanted by multiplying each of the





rather too small ; but on examining with attention all the corrections which the authors were obliged to make on the immediate results of their observations, it is easy to see that the rapidity with which hydrogen gas cools down to the temperature of the surrounding bodies, compared with other elastic fluids, ought necessarily to introduce into the determination relative to that gas an inaccuracy from which they did not attempt to free it. By taking into consideration this cause of error, we are enabled to explain the difference alluded to without being obliged to make any forced supposition.

The law of specific heats being thus established for elementary bodies, it became very important to examine, under the same point of view, the specific heats of compound bodies. Our process applying indifferently to all substances, whatever be their conductivity or state of aggregation, we had it in our power to subject to experiment a great many bodies whose proportions may be considered as fixed ; but when we endeavour to mount from these determinations to that of the specific heat of each compound atom by a method analogous to that which we employed for the simple bodies, we find ourselves soon stopped by the number of equally probable suppositions among which we must choose. If the method of fixing the weight of the atoms of simple bodies has not yet been subjected to any certain rule, that of the atoms of compound bodies has been, *à fortiori*, deduced from suppositions purely arbitrary. But instead of adding our own conjectures to those which have been already advanced on the subject, we choose rather to wait till the new order of considerations which we have just established can be applied to a sufficiently great number of bodies, and in circumstances sufficiently varied that the opinion adopted may be founded on decisive conclusions. We shall satisfy ourselves with saying, that in abstracting every particular supposition, the observations which we have hitherto made tend to establish this remarkable law ; viz. that there always exists a very simple ratio between the capacity of the compound atoms and that of the elementary atoms.

We may likewise deduce from our researches another very important consequence for the general theory of chemical actions, that the quantity of heat developed at the instant of the combination of bodies has no relation to the capacity of the elements ; and that in the greatest number of cases this loss of heat is not followed by any diminution in the capacity of the compounds formed. Thus, for example, the combination of oxygen and hydrogen, or of sulphur and lead, which produces so great a quantity of heat, occasions no greater alteration in the capacity of water or of sulphuret of lead than the combination of oxygen with copper, lead, silver, or of sulphur, with carbon, produces in the capacity of the oxides of these metals, or of carburet of sulphur.



state. Some facts more lately observed could not be explained according to the theory without admitting that the oxygen contained in certain combinations retained a quantity of heat superior to that which it contains when in the elastic state. Such are the detonations produced by mixtures of chlorate of potash with certain combustibles, or the spontaneous explosions of the euchlorine of Davy, and of the chloride and iodide of azote.

This explanation was afterwards extended to all combinations, and it was considered as a principle sufficiently established that a body in combining with a certain number of others might abandon a more or less considerable part of its heat, according as in each case the different degrees of affinity of the elements in contact occasioned the molecules to approach more or less nearly to each other. It is the degree of this approach, essentially variable, which has been denoted by the word *condensation*, so frequently employed in the language of chemistry.

Such is the theory almost generally adopted in France. Several foreign chemists have pointed out its inaccuracy, and have modified it in different points, but without producing any conclusive proof either against the opinion which they combat, or in support of that which they wish to substitute in its place.

We see then that the different explanations relative to the development of heat in chemical combinations are reducible to simple assertions derived from the first hypothesis of Lavoisier. It is astonishing that since the time in which this doctrine originated, it has not been subjected to a more rigid examination; and that even from the results already known, all the arguments have not been drawn against it which they are capable of furnishing. We conceive that the relations which we have pointed out between the specific heats of simple bodies and those of their compounds prevents the possibility of supposing that the heat developed by chemical actions owes its origin merely to the heat produced by changes of state, or to that supposed to be combined with the material molecules. We have still a better reason to reject this purely gratuitous hypothesis, as we can explain the phenomenon in a much more satisfactory manner.

In fact Davy has long ago shown that when the two poles of a voltaic pile are united by means of pieces of charcoal placed in a gas incapable of supporting combustion, the charcoal may be kept in a state of violent ignition as long as the pile remains in activity, and without the charcoal undergoing any chemical change. On the other side, we are warranted to conclude from a great number of galvanic experiments made by Hisinger and Berzelius, and by Davy, that all bodies which combine are, with respect to each other, at the moment of combination precisely in the same electric conditions as the two poles of the pile. Is it not then probable that the cause which produces the incandescence of the charcoal in the fine experiment just mentioned is likewise the cause of the greater or less elevation of tempera-



Humphry tells us the papyri of Herculaneum were never acted on by fire, and that the consolidation of the tuffa and infiltration of water have converted them into a state analogous to peat or Bovey coal. To those who have seen the MSS. at the Studii of Naples, a similar conclusion must be obvious. The brown colour of the papyrus contrasted with the jet-black letters of the MSS. leads us to believe that the temperature at least has not been sufficient to convert it into a perfect charcoal.

Nothing can be more grossly absurd than the idea which refers the destruction of Herculaneum to a *wave of the sea*. Such persons tell us that *lapillo* is constantly forming on the shores of the gulphs of Naples similar to that which ruined the cities of Herculaneum and Pompei. I can, however, assure you that there is no such exhibition. In descending from the declivity of Monte Somma to the *Fosso grande*, I passed through a section of ashes which I consider to be the very same with those which overwhelmed the two celebrated cities in question. The following is the opinion of Cheval. de la Condamine (see Remarks, &c. 1768): "The substance which fills the inside of the city was never melted nor liquid; but is an immense amassment of ashes, earth, gravel, sand, coal, pumice stones, and other substances, launched up from the mouth of the volcano at a time of its explosion, and fallen down all around it. These at first buried all the buildings, and afterwards by degrees got into the inside of them by their own weight, and the drift of winds and rain, and lastly, by the falling in of the roofs and floors. This mixture, clung together by the infiltration of waters, became condensed by time, forming a kind of sand-stone, more or less hard, but easily penetrable." I was particular in my observations of Herculaneum, and the opinion I formed of the phenomenon was precisely that of Sir H. Davy, so corroborative of that of de la Condamine. I also compared this tuffa with that in other parts around Vesuvius, and their identity is not with me a subject of question. I might add, that the superior proximity of Herculaneum compared with Pompei would subject the former to torrents more dense and terrible than those lighter materials which would be transported toward Pompei. We are informed by Tacitus, that the volcano in the dreadful eruption of 79 changed its scite and aspect; and Horace relates that then Vesuvius was rent in two places, and discharged immense torrents of flame. Herculaneum is in a very humid situation compared with Pompei, and is much more exposed than that city to infiltration of water. Its present very moist condition proves the fact. Perhaps too, torrents of mud and water, as frequently occur in the volcanoes in the range of the Cordilleras, may have issued from one of the craters formed; and though it might inundate Herculaneum, could not be carried as far as Pompei. I must, however, suppose that the ashes, though not incandescent, were of a highly elevated temperature.





40° 44' 59.93". His repeating circle was planted in the temple of Isis.

I am not aware that the method of making the *papyri* from the *cyperus papyrus* is generally known. Modern *papyri* have been lately made by Cavaliere Landulina, at Syracuse, in Sicily. Thin fresh slices of the *medulla* or pith are brought in contact transversely, and the whole is subjected to considerable pressure when the edges cohere.

Sir H. Davy does not consider it proper to publish the method he adopts, or what he employs; but I think it is clear, from a simple inspection of the Herculean MSS. that concentrated ether, or even absolute alcohol, might be employed with success.

I have the honour to be, Sir, your most humble servant,

J. MURRAY.

## ARTICLE VI.

*Descriptions of the new Species of Animals discovered by his Majesty's Ship Isabella, in a Voyage to the Arctic Regions.*  
By Dr. W. E. Leach.\*

### Type VERTEBROSA.

#### Class MAMMALIA.

##### Genus *Canis*, of authors (Dog).

A variety approaching to the wolf in many points of external character and in voice, was found in a domestic state amongst the inhabitants of Baffin's Bay. The great toe on the hinder feet is wanting. Dr. Blainville supposes it to be the origin of the wolf-dog (the chien-loup of the French).

##### Genus *Lepus*, of authors (Hare).

Species *Glucialis*. Albus, vertice et dorso pilis nigricantefuscis albo-fasciatis sparsis, collo lateribus nigricante alboque mixtis, auribus apice extremo nigris.

This animal, which will neither agree with the *Lepus albus* of Brisson, nor the *Lepus variabilis* of Pallas, both of which are now before me, is of the size of the common hare (*Lepus timidus*), and of a white colour. The back and top of the head are sprinkled with blackish-brown hair, which is banded with white; the sides of the neck are covered with hairs of the same colour, interspersed with white. The extreme tips of the ears are tipped with black, intermixed with white; the insides of the ears have a few black hairs mingled with the white.

\* From "A Voyage of Discovery made under the Orders of the Admiralty, in his Majesty's Ships Isabella and Alexander, for the Purpose of exploring Baffin's Bay, and inquiring into the Probability of a North-West Passage By John Ross, K. S. Captain, Royal Navy." Second Edition. In Two Volumes.



Species 1. *Arctica*, purpurascente-carnæa tenuiter striolata, operculo testaceo.

Baffin's Bay, Capt. Ross, Capt. Sabine.

Species 2. *Striata*, anfractibus longitudinaliter striatis et obliquè antiquatis.

Baffin's Bay, Mr. Beverly.

Genus *Natica* (Lamarck).

Species 1. *Beverlii*, spira elevatiuscula, anfractibus superioribus convexiusculis.

Baffin's Bay, Mr. Beverly.

Species 2. *Fragilis*, spira ferè obsoleta, testa fragillissima, operculo hyalino.

Baffin's Bay, amongst the soundings taken up by Capt. Ross's instrument.

Genus *Buccinum*, of authors.

Species 1. *Boreale*, purpurascente-brunneum, anfractibus cancellato-striolatis, supra abbreviato costatis, lineis prominulis 1-canaliculatis spiraliter ascendentibus.

Baffin's Bay, on Hare Island, Mr. Beverly.

The canal of the anterior part of the shell is of a moderate length.

Species 2. *Rossii*, anfractibus tribus basilaribus transversim costatis: tertio costis supernè imperfectis, anfractibus apicalibus simplicibus glabris.

Baffin's Bay, Mr. Beverly.

This species resembles at first sight *Buccinum Bamffium* (murex Bamffius, Donovan), but it may readily be distinguished by the number of costated whirls, *Bamffium* always having the four basal ones ribbed.\*

#### Class CONCHÆ.

##### Family I.—PHOLADIDÆ?

Genus *Pholeobia* (Leach).

Char. Testa elongata, porticè clausa anticè hians: cardo edentulus; Ligamentum exterius prominens.

Species *Rugosa*.—*Myrtillus rugosus*, of English authors.

##### Family II.—MYADÆ?

Genus *Pandora* (Lamarck).

Species *Glacialis*, anticè rotundata obtusa, dentibus cardinalibus crassissimis. Taken up with soundings in Baffin's Bay. Received also from Spitzbergen.

\* Mr. Beverly communicated to me an *Achatina*, which he found on an island in Baffin's Bay, and as it is a tropical genus, I cannot refrain from noticing so extraordinary an occurrence.



## Class BRACHIOPODA (Cuvier).

Genus *Terebratula* (of authors).Species *Substriata*, testa radiatim et concentricè striolata.

Lat. 76° N.; long. 76° 55' W. amongst soundings.

Type ANNULOSA.

## Class CIRRIPEDES (Cuvier).

Genus *Balanus* (of authors).

Species *Arcticus*, testis costato-elevatis: costis irregularibus rudis, interstitiis lamellato-striatis. Baffin's Bay, on the rocks, common, Mr. Beverly. Unfortunately the operculum of this fine species was lost.

## Class CRUSTACEA.

Genus *Hippolyte* (Leach).

Species ——. Having mislaid the specimen, I cannot give a description of it.

Genus *Gammarus* (Latreille).

Species 1. *Sabini*, segmentis dorsalibus posticè falcato-productis.

Baffin's Bay, Capt. Sabine.

## Class ANNELEIDES (Cuvier).

Genus *Nereis* (Linné).

Species *Phyllophora*, ore edentulo, pedibus basi lamellis foliosis instructis. Baffin's Bay.

Genus *Lepidonotus* (Leach).

Species *Rossii*, pedibus densissimè testaceo-hirsutis, squamis dorsalibus cærulescente-griseis. Baffin's Bay, amongst soundings.

*Dentalium*, of authors.

Species *Striatulum*, subcurvum longitudinaliter elevato-lineolatum.

Lat 62° N.; long. 62° amongst the soundings.

Type RADIATA.

## Class ECHINODERMATA.

Genus *Gorgonocephalus* (Leach), 1815. *Euryale* (Lamarck), 1816.

Species *Arcticus*, corpore supra glabro radiatim costato: costis





cistern was void of water, and that when water was placed in the safety cell, it was enlarged. It requires also to be occasionally replenished; and I could not find that the small portion driven by the retrogression of the flame into the reservoir was any equivalent for the quantity supplied. When oil is made to substitute the water, it too disappears, the flame is not so intense, and carbonaceous matter is found in contact with the escape pipe; in both cases there is probably a partial decomposition; hence an attenuation of intensity of flame. When oil is used, carbonic oxide or carburetted hydrogen would certainly, by mingling with the condensed oxygen and hydrogen, have this direct tendency.

You are aware that about two miles from Pietramala there is a constant evolution of carburetted hydrogen, described so long ago as 1776 by J. J. Ferber, and the following are his words: "On the sloping side of a hill towards the valley appear continual flames, which, being ever to be seen, have caused this hill to be called *Pietramala*. The flaming place is covered with earth, and loose separate lime, clay, and marl stones. It has properly but six feet diameter, and the flames appear between and upon the before-mentioned stones. The flames are exceedingly clear and are yellowish-white as arising from burning oil. They rise about two feet above the ground, give no mark of any sulphureous acid, grow stronger after wet weather, and fainter in a dry summer." These extracts are quoted to show that this exhibition is not a new discovery.

When I crossed the Apennines on my journey to Florence, I was naturally anxious to visit so curious a phenomenon. The few experiments which I was enabled to make convinced me that the flame was continued by a constant evolution of pure carburetted hydrogen. The flame was only a *few inches high*, the weather for some time before had been particularly dry, and continued so for many days after; when the breeze agitated the flame, propelling it in a given direction, it exhibited that *fine blue* colour which appears when the bright flame of supercarburetted hydrogen is acted upon by a current of air. By placing a small glass bell over a confined portion of it, I obtained a quantity of the gas, which burned with a yellowish-white flame in an inverted vessel, when ignited in contact with the atmosphere, and it detonated violently when mixed with a due proportion of that medium. What particularly struck me was the remarkable *etherial smell* which accompanied this extraordinary and perpetual combustion. When I returned from the south of Italy, I was desirous of revisiting this 'volcanello' (as the natives call it), and examine the hygrometric state of the atmosphere in reference to it, as I had done before. This anticipated pleasure, however, it was not my good fortune to realize. The snow was three and four feet deep even on the road, and our "vetturino" was necessitated to effect his progress by harnessing four oxen



nearly a *white heat*, the gas burns with a *clear bluish flame*.

There is a phenomenon which seems still involved in considerable mystery, and which the chemist has not been able as yet satisfactorily to explain—I allude to the decomposition of *ammoniacal gas*. This gaseous body suffers no change, even when transmitted through an incandescent porcelain tube. But if several coils of iron wire be introduced, the gas suffers decomposition. It is effected also by copper, &c. but *not entirely by platinum*. The metal suffers no chemical change in consequence, but a remarkable physical one; it becomes *extremely brittle*, so as to be easily crushed between the fingers. It has struck my mind that the phenomenon adverted to, and the aphlogistic exhibitions of platinum, silver, &c. are similarly related, and may probably be traced to the same cause or source. Some of the metals exhibit aphlogistic effects, and some do not, and there are metals which decompose ammoniacal gas, and others which do not. Iron, &c. are found extremely brittle after they effect the separation of this compound gas into its elements, and silver wire, when it has shown its aphlogistic powers for some time in inflammable vapour, also becomes brittle.

I have the honour to be, Sir,

Your most humble and very obedient servant,

J. MURRAY.

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### ARTICLE VIII.

#### *New Results on the Combination of Oxygen with Water.\**

By M. Thenard.

I HAVE at last succeeded in saturating water with oxygen. The quantity which it then contains is 850 times its volume, or twice as much as the quantity that belongs to it. In that state of saturation, it possesses properties quite peculiar, the most remarkable of which are the following:

Its specific gravity is 1.453. Hence when it is poured into common water, we see it fall down through that liquid like a sort of syrup, though it is very soluble in it. It attacks the epidermis almost instantly, and produces a prickling pain, the duration of which varies according to the quantity of liquid applied to the skin. If this quantity be too great, or if the liquid be renewed, the skin itself is attacked and destroyed. When applied to the tongue, it whitens it likewise, thickens the saliva, and produces on the organs of taste a sensation difficult to express; but which approaches to that of tartar emetic. Its action on oxide of silver is exceedingly violent. Every drop of the liquid let fall

\* Translated from the *Annales de Chimie et de Physique*, x. 335.



novelty adds an exquisite seasoning to every investigation, it is not sufficiently powerful of itself, at least in most persons, to continue this activity during the whole period of human existence. Curiosity becomes more and more blunted as we advance in life. Objects which once possessed for us the most exquisite charms, when stripped of the gay colouring thrown over them by novelty, cease to excite our attention. He who surveys the same prospect every day, however finished the landscape may be, gradually neglects it, or sees it without emotion. Hence the importance of the love of distinction, a desire which seldom palls upon the senses, but generally continues equally strong, if it does not in reality rather increase in strength to the latest period of life. Were a man confined for life in a desert island, it is not likely, however great abilities he possessed, that he would devote much of his time to the cultivation of knowledge. Even an Archimedes in such a situation would relinquish his calculations. What gratification could he derive from his discoveries when he had no one to communicate them to, no one to admire them, and when he had the mortifying certainty that they would perish with himself? Man, as far as his improvement in knowledge is concerned, is essentially a social being.

Hence it is of the greatest consequence for the progress of any science when it becomes popular; when it excites the attention of a great number of individuals; when it becomes a general object of study. The number of real improvers of it, perhaps, is not greatly augmented; but the theatre on which they act is greatly enlarged. A greater degree of emulation is excited; a more energetic activity is ensured. Their labours are more accurately appreciated, and more liberally rewarded; and they are assured of that reward which possesses for them the greatest of all charms; they are raised to distinction among their contemporaries, and handed down to posterity with immortal honour.

I have been induced to make these observations from the nature of the work, the title of which stands at the head of this article. Its object is not to enlarge the bounds of chemistry, nor to introduce any new views deduced from facts already known; but to render chemical experiments a source of amusement, and thus to draw even the idle and the indolent to this brilliant and fascinating science. The intention is laudable, and we sincerely hope that the author has succeeded in his object. The experiments selected are sufficiently varied and striking to constitute a very amusing exercise to young men; while they are almost all easily performed, as neither costly materials nor expensive apparatus are required for them. The descriptions of them are distinct, and most of the explanations satisfactory. The experiments are 103 in number; and being all unconnected with each other, it is impossible to convey any idea of them to the reader except by transcribing one or two of them by way of specimen.



*Exper. 20. A colourless Fluid, which acquires a blue Colour when the Bottle containing it is opened, and which becomes limpid again when the Bottle is closed.*—Put into an ounce phial a slip of copper, previously scraped bright: fill up the phial with liquid ammonia, and cork it air-tight. No apparent change will take place; but if the bottle be left open for some hours, and then be closed, a solution of the colour is effected, which is absolutely colourless, but turns blue on re-opening the bottle, beginning at the surface, and gradually extending downwards through the mass. Again, if this blue solution has not been too long exposed to the air, and fresh pieces of copper be put in, stopping the bottle again, the solution is deprived of all its tinge, and recovers its colour only by the admission of the air. And this effect may be produced repeatedly.

*Rationale.*—Metallic copper is not acted upon by liquid ammonia, but is soluble in this fluid when oxidated. This oxidation is effected by the influence of oxygen when atmospheric air is admitted. Hence, when the copper is no further oxidated than is necessary for solution, the compound is colourless; but it acquires a blue colour when the metal is oxidated in a higher degree. It is also obvious, that this azure colour is again destroyed by the addition of more copper filings, and exclusion of air, as the newly added metal deprives that which was contained in the solution in an oxidated state, of its superabundance of oxygen, in order to be dissolved also in the liquid. The solution of copper in ammonia, at a minimum of oxidation, is, therefore, colourless; but if the metal be highly oxidated, the solution has a blue colour.

*Exper. 21. To prove that Water is contained in the Air of the Atmosphere, even during the driest Weather.*—Take a tea-spoonful of dry muriate of lime, acetate of potash, or sub-carbonate of potash, put it into a saucer or other vessel, and suffer it to be exposed to the open air for a few days. The dry salt will thus be rendered completely liquid by the watery vapour which always exists in the atmosphere.

The proportion of watery vapour existing in the atmosphere varies considerably, principally according to temperature. While the water preserves the aerial form, the air containing it is perfectly-transparent; even in this state, however, it can be discovered existing in it; but when the vapour is condensing, it communicates to the air a degree of opacity from the conglomeration of the particles of water. This, according to the extent to which it happens, gives rise to the natural appearances of clouds, mist, and rain.

*Exper. 22. To write luminous Characters.*—Write with a stick of phosphorus on a board, or on any rough surface: the characters will be luminous in the dark, as if on fire, and continue so for some time. The luminous appearance vanishes by blowing on the writing, and becomes visible again instantly.

If letters be written on a dark-coloured paper, and the writing

be held near the fire, the characters instantly inflame, and exhibit a beautiful phosphorescent appearance.

*Rationale.*—This effect is nothing else than the slow combustion of the minute abraded particles of phosphorus, effected by the oxygen of the atmosphere.

N. B. Phosphorus should always be handled with the greatest caution, for serious burns have happened from carelessness in this respect to persons getting small pieces of phosphorus under their nails. It is best to place the phosphorus in a quill or glass tube, that it may be removed from the hand in case it should take fire: a bowl of water should also be near at hand to plunge it into in case of accidents.

\* \* \* The above remarks apply to the first edition of the *Chemical Amusement*; but such has been the rapid sale the work has experienced, that it has already arrived at the *fourth* edition. The author has taken advantage of this circumstance, and materially improved it. The number of experiments is not only increased from 103 to 147, but much interesting matter connected with chemistry in general has been likewise added.

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## ARTICLE X.

### *Proceedings of Philosophical Societies.*

#### LINNEAN SOCIETY.

*May 4.*—A paper, by Charles Hamilton Smith, Esq. A. L. S. was read, entitled “Observations on some Animals of America, allied to the Genus Antelope,” accompanied by figures.

At this meeting was also read, “Characters of a new Genus of Coleopterous Insects of the Family Byrrhidae,” by W. E. Leach, M.D. F.L.S.

*June 1.*—Was read, “Descriptions of some Shells found in Canada,” by the Rev. Thomas Rackett, F.L.S.

Also “Observations on some migrating Species of Sylvia,” by Mr. R. Smith, F.L.S.

*June 15.*—Part of a paper, by J. Murray, Esq. was read, entitled “Remarks on some of the natural Phenomena around Naples, and on Botany considered as an Auxiliary to the Geologist.”

#### GEOLOGICAL SOCIETY.

*May 7.*—The “Description of the Valley of the Ligovca,” by the Hon. W. T. H. F. Strangways, was read.

The river Ligovca, or Doodorovca, issues from the lake Doodorof, which is about 15 miles to the south-west of Petersburg. At a short distance from this lake, it is expanded into a second, and finally discharges itself into the gulph of Finland, through a marsh, which is daily increasing in extent. In the upper part of its course, the bed of the river is composed of the limestone,



*May 21.*—The Secretary gave notice that the following communication had been received since the last meeting: "Observations on the geological Relations of the Environs of Tortworth, Gloucestershire; and of the Mendip Range in Somersetshire," accompanied by a Map and Sections, by T. Weaver, Esq. M.R.I.A. M.W.S.

A paper, by the Right Hon. Lord Compton, was read, containing "A Description of the Rocks which occur along a Portion of the South Coast of the Isle of Mull."

These, which are called the Carsey rocks, and consist of high precipices, which are sometimes close to the sea, and at other times recede a little from it, are composed almost entirely of basaltic columns. In several parts they are intersected by whin dykes, of which there are three that are very remarkable; they are almost 120 feet in height, and varying in breadth from  $5\frac{1}{2}$  feet near the bottom to about 17 at the top; they are situated near together; the first is nearly perpendicular, but the other two are inclined considerably to the east. The basaltic columns which compose the shore are, in some places, as much as 500 feet in height. In one part, a group of these columns rises in an insulated form out of the sea to the height of 70 feet.

There are two very remarkable arched rocks; one of the openings is about 60 feet high, and between 50 and 60 wide; it is formed in a basaltic rock resting on green sand, and does not contain any fossils; the rock itself is from 110 to 120 feet high, and there is a stratum of basaltic columns above the arch. The other arch is rather higher, but considerably smaller in its other dimensions. The part of the shore to which the arched rocks are attached is composed of lofty basaltic columns.

The author states that in different parts of the basalt which forms the coast, he found crystallized carbonate of lime, chalcidony, quartz, different kinds of zeolite and analcime, as well as a mineral which is supposed to be pitchstone.

The reading of Mr. Taylor's paper "On the Smelting of Tin Ores in Cornwall and Devonshire," was begun.

*June 4.*—The reading of Mr. Taylor's paper "On the Smelting of Tin Ores in Cornwall and Devonshire," was concluded.

The author observes that tin ore is found in two states, in veins accompanied by other metals, or in detached fragments dispersed through alluvial matter; they are known by the names of mine tin and stream tin respectively. Mine tin is first subjected to the process of dressing, by which a considerable part of the extraneous minerals, as well as the earthy matrix, is separated. The metal produced from this kind of ore is called block tin, and is less pure than that from stream tin, in consequence of some remains of other metallic substances, of which it is very difficult entirely to deprive it. Stream tin has no other metallic ore mixed with it, except occasionally a little hæmatitic iron. This furnishes the grain tin of commerce.



theorems and the most important consequences. The title of the first paper is: "On the Rotation of the Earth."

From the time of Hipparchus to the present, the length of the day has not changed the hundredth part of a second. The axis of the earth's rotation is thus as invariable in relation to its surface as the quickness of the rotation; the axis always answering to the same points of the earth, the most exact observations have not discovered any change in the geographical latitude of places. A century ago, Cassini endeavoured to demonstrate this truth, but he did not dare to affirm it absolutely. He only simply stated, that if any variation of the height of the pole existed, it must be extremely small. At present it may be asserted for a certainty, that the earth moves uniformly round an invariable axis.

It is known that all solid bodies have three principal rectangular axes, around which they can revolve in an uniform manner, the axis of rotation remaining at rest. Is this very remarkable property common to bodies which, like the earth, are surrounded by a fluid? The actions of the sun and moon influence the figure of the sea, which, by these means, varies incessantly. Among the powers which produce the phenomena of the tides, some are variable; but as these are beyond comparison much less than the centrifugal force, the alteration which they produce in the permanent figure of the earth is insensible. A small agitation in an ocean of quicksilver, if it were to be substituted for our present seas, would be sufficient to spread it over the terrestrial continent. This well-known inferiority of the density of the sea is the consequence of the original fluidity of the earth; for at that time the heaviest strata were enabled to settle nearest the centre. Theoretical considerations unite with the experiments made upon the pendulum to show that, in all probability, it was a very violent heat which rendered all the parts of the earth originally liquid.

"The laws of mechanics and of gravity are, therefore, sufficient to give a firm state of equilibrium to the sea, which is but very slightly altered by celestial attractions. Its gravity, which constantly tends to bring it back to a state of equilibrium, and its specific gravity less than that of the earth, both necessary consequences of these laws, are the true causes which maintain it in its limits, and hinder it from spreading itself over the land, which is a necessary condition towards the preservation of organized beings. The necessity of this condition may appear to be a sufficient reason for its existence, but these kinds of explanation ought to be banished from natural philosophy, as they would infallibly hinder its progress. The phenomena ought to be confined as much as possible to the laws of nature, and when this object cannot be obtained, we ought to stop; always recollecting that the true progress of philosophy consists in proceed-







a degree for Jupiter, and to four-fifths of a degree for Saturn, and affects the whole system through the action of these two large masses. Fortunately the coefficients of the inequalities produced by this cause in the elements of the planetary motions are insensible; they only amount to about one centesimal second for Mars and Uranus; to six-tenths of a second for the earth; and to one centesimal second for the moon. A more sensible effect is produced upon the satellites of Jupiter. In this case the coefficients are, in centesimal seconds,  $1''$  for the first satellite,  $12.8''$  for the second,  $18.8''$  for the third, and  $44.3''$  for the fourth. Still more fortunately the motion of these satellites is so rapid, that as these four coefficients are, like the former ones, fractions of a degree, and not of time, it may therefore be said that these inequalities are lost in the uncertainties of observations. They do not alter in the least the remarkable ratio which exists in the motions of the first three satellites.

The third memoir is entitled, "On the Law of Gravity, supposing the terrestrial Spheroid was homogeneous, and of the same Density as the Sea."

Upon the hypothesis of the homogeneousness of the terrestrial spheroid, analysis reduces the gravity on the surface of the sea to a very simple expression, and one that offers a remarkable result: namely, that if the sea was of the same density as the spheroid, the force of gravity on its surface would be independent of the figure. Taking any point situated either on the surface of the sea, or on a continent, or an island, the force of gravity would be equal to a constant quantity, plus the product of the square of the sine of the latitude by five-fourths of the ratio between the centrifugal force and the gravity at the equator, minus the product of the gravity at the equator by half the height of the given point above the level of the sea, a height which might be determined by the barometer; the mean radius of the earth is here to be taken as unity.

As this law does not agree with the experiments on pendulums made in both hemispheres, the hypothesis of the homogeneousness of the earth is, therefore, shown by these experiments to be false: at the same time they prove,

1. That the density of the strata of the terrestrial spheroid increases from the surface to the centre.

2. That these strata are disposed very nearly in a regular manner round the centre of gravity of the earth.

3. That the surface of this spheroid, covered in part by the sea, has a figure slightly differing from that which, if it were fluid, it would assume by virtue of the law of equilibrium.

4. That the depth of the sea is but a small fraction of the difference between the two axes of the earth.

5. That the inequalities on the surface of the earth, and the causes which occasion them, have very little depth.

6. That the whole earth was originally fluid.



very ingenious machine, but this cannot give the least idea of its quantity. Indeed analysis itself only shows an approximation to this; and for a long time there will be only astronomical observations to determine the motion of the precession with sufficient exactness.

*On the Libration of the Moon, by M. Poisson.*—"According to the laws of this phenomenon, discovered by D. Cassini, and confirmed by the beautiful calculation of M. Lagrange, the moon revolves upon her axis in the same time that she accomplishes her mean revolution round the earth: her equator preserves a constant inclination upon the ecliptic, and the descending node of this equator coincides with the mean ascending node of the lunar orbit. M. de Laplace proved that these results are not affected either by the secular equation of the mean motion of the moon, nor by the secular displacing of the ecliptic; it is also certain that they are not altered by the secular equation that affects the mean motion of the moon's node; but these results agree only with the mean velocity of rotation, and with a mean state of the lunar equator; and theory shows that this velocity of rotation, the inclination of the equator, and the distance of its node from that of the orbit, are subject to periodical inequalities, whose *maxima* depend upon the ratio among the momenta of the moon's inertia. M. Lagrange gave the expression of the principal inequalities of the velocity of rotation; so that in order to render the theory complete there remained only to determine the inequalities of the inclination and of the node. This is what I propose to determine, by taking up afresh the solution of the problem, and by carrying on the approximation unto the terms of the second order in respect to the elements of the lunar orbit, which terms contain the inequalities in question. I shall confine myself to giving the formulæ I have discovered, and shall suppress the details of the calculations which led me to them, and which are only a development of the calculation of M. Lagrange."

The latter end of this paragraph does not mean that no formulæ are to be found in the memoir, they being, on the contrary, indispensable to show the changes which take place in the expressions by the introduction of terms of the second order. The author considers successively the different inequalities of the longitude of the node: the second is known, it is about a fifty-fifth part of the mean inclination. He shows that the first is less than a twenty-seventh part of the same inclination. Two similar inequalities are to be found in the distance of the node of the equator from that of the orbit. By the second, the two nodes are separated from one another by more than a degree; the maximum of the first does not surpass two degrees.

M. Bouvard found that the distance of these nodes is  $2^{\circ}$ ; Mayer found it to be  $4^{\circ}$ , but in a contrary way. The difference betwixt these two results may be partly attributed to the errors



the final result will be as exact, and the demonstration as well established, as if these irrational values had not been employed.

From the very simple analysis of the author, it also follows, that if the general formula of the roots of unity does not contain radicals, the exponents of which will not divide  $(p - 1)$ , then there is not, in the whole expression, a single radical which is not related to an exact power of the same degree, or rather to a residual of that power; so that, by the restitution of certain multiples of  $p$  to these residuals, the expression will become, in all its parts, commensurable and entire, and will not show in any part any sign of an impossible operation.

But if there are found in the formula roots which are not divisors of  $(p - 1)$ , there will be in it, under these radicals, numbers which will not be residuals of powers of the same degree, and consequently the formula will contain irrational quantities which can never be reduced to entire numbers in respect to  $p$ . But these irrational quantities may be exact powers of irrationals of the same form, so that the radical operation that is pointed out can be executed; and then, in the addition of these similar values, the incommensurables are destroyed of themselves, and the formula will always lead with equal precision to the entire roots of the proposition, when this equation will admit such roots.

These are the principal points of this remarkable theorem, which offers, as the author observes, the first and only example of the application of algebra to the theory of numbers.

M. Poincot has examined this theorem still more deeply, and explained it by a multitude of examples, which exhibit a number of curious theorems concerning the residuals of the powers of the superior degrees. He also applies it to the determination of the primitive roots of prime numbers; and lastly, he extracts from it some general truths in respect to algebra, which were apparently impossible to be discovered by any other method.

As to the rest, this theorem of the binomial equations extends to any equation, the algebraic resolution of which shall be esteemed as known. The author points out, in a few words, this general demonstration at the commencement of his memoir; "but I wished," he says, "to study more particularly the binomial equations, because they are, as it were, the key of all the others, because they alone are able to show the intimate nature of roots: those remarkable signs, which exhibit the essence of algebra by that equivocation of the different senses in which they may be taken, and the employment of which, in the chain of mathematical reasoning, exhibits the most precise difference between analysis and synthesis."

The author proposes to proceed with this curious approximation of algebra, and the theory of numbers, and to show that the general principles of algebra, properly so called, have their origin in the simple consideration of the order, or of the mutual dispo-





duced by the integration of these five differential equations. Now the method which I propose consists in avoiding the introduction of these constant quantities, or rather in supplying the place of these constant quantities by particular values, attributed to the unknown quantities  $y, z, u, q, r$ , and in integrating the five differential equations, in such manner that for  $x = x_0$ , we may have  $y = y_0, z = z_0, u = u_0, q = q_0, r = r_0; y_0, z_0$ , designating two new variables;  $u_0$  an arbitrary function of these variables themselves, similar to the arbitrary function of  $y$  and of  $z$ , which represent the value of  $u$  for  $x = x_0$ ; and  $q_0, r_0$ , the two partial derivatives of  $u_0$  relative to  $y_0$  and to  $z_0$ . If, in the five integral equations thus obtained,  $q$  and  $r$  are eliminated, there will remain only three formulæ, the system of which will be proper to represent the general integral of the equation with partial differences. These three formulæ will contain the variable quantities  $x, y, z, u$ ; the constant quantity  $x_0$ , the two new variable quantities  $y_0, z_0$ , and the arbitrary function of these new variable quantities represented by  $u_0$ , as well as its derivatives of the first order relative to  $y_0$  and to  $z_0$ . It is not until the arbitrary function in question has been fixed that by eliminating the new variable quantities  $y_0, z_0$ , we can obtain the limited equation which determines  $u$  in a function of  $x, y, z$ .

Nothing hinders us from preserving in the calculation the quantity  $p$  along with the variable quantities  $x, y, z, u, q, r$ , if it is observed besides, that the independent variables,  $x, y, z$ , may be exchanged for one another relative to the parts which they act, the following rule will be obtained by the general integration of an equation with partial differences for three independent variable quantities, and even for any number whatever of variable quantities.

Substitute, according to the ordinary methods, in the place of the given equation with partial differences, so many differential equations of the first order (minus one) as it contains variable quantities, comprising therein the independent variations, the unknown function, and its partial derivatives. The independent variable quantities are to be treated symmetrically in the differential equations, one of which may be supplied by the equation with given partial differences.

This being done, integrate the differential equations in question relatively to all the variable quantities that they contain, setting out from certain limits which you consider as new variable quantities, subjected to the same relations as the first. Then, in the integral equations thus obtained, regard one of the new independent variable quantities as being reduced to a constant quantity, and the others as what ought to be eliminated, you will have a system of formulæ proper to represent the general integral of the equation with given partial differences. These formulæ will contain only one arbitrary function, with its partial divisions of the first order; that is to say, the new variable



isochronous vibrations, which run on together without interfering with one another; but the relations are not the same as for flexible strings: the octave, the twelfth, the seventeenth, are not heard. This resonance then is not a general fact serving as the foundation of the laws of harmony. According to the suppositions which may be made, and the arbitrary functions which may be introduced into the calculation, a great number of partial sounds may be suppressed at the beginning. Thus in a case where the author lays down all the circumstances, the subordinate sound, the lesser sharp, will occur at the triple octave of the second major of the principal sound; an interval which is looked upon as dissonant: the superior sounds, could not be determined in the least.

If an extended flexible surface of a rectangular figure, the extremities of which are fixed, is considered, the movement may be resolved into a multitude of partial movements, each of which is expressed by a particular integral: the coefficients of the different terms are limited integrals easy to be obtained, the series being convergent; the subordinate sounds have not in general any commensurable relation: these sounds are totally different from those produced by an elastic surface and the monochord. In the case of there being only one dimension, the flexible body is sonorous, the harmony is pure and complete; but as soon as a second dimension is added, all harmony ceases, and there is nothing but a confused mixture of sounds, only slightly distant from each other, and of which it is impossible to discern the relation.

If the surface is elastic, the equation, instead of being one of the second order, is of the fourth. The subordinate sounds are to one another, and to the principal sound, as one number to another; and it is upon this account that elastic surfaces yield harmonious sounds.

If constant retarding forces are made to enter into the calculation, the tone remains the same, but the sound weakens, the motion ceases, or rather it passes into the neighbouring bodies, and is propagated in them. The action of these forces rapidly destroys the accidental effect of the initial disposition, and leaves only the effect of the proper elasticity and the figure of the sonorous body in action for some time.

If the elastic surface, being of a very small thickness, has its other dimensions unlimited, the movement is propagated rapidly along the whole extent of the surface; foldings and annular furrows are formed, which recede from the origin of the motion. The question will then be to express in a single formula all the variable states of the surface, so that its figure at any instant of time may be exactly determined. This equation contains, under the double sign of definite integration, two auxiliary variable quantities along with the three principal variable quantities. The



and soon ceases to be observable. The same results take place in the apparent motions of sonorous strings. This effect is comparable to that of the diffusion of heat in solid matter.

"We now terminate the account of our researches on the motion of elastic surfaces. They furnish new proofs of the extent of that mathematical analysis whose principal object is the interpretation of natural phenomena. This science expresses, in simple forms, the most complicated natural effects; it shows us those which subsist far from us at immense distances, and those which will only be accomplished in future times, or which have preceded us for ages; it determines the general and simple laws which regulate all the motions of heat, or the harmonious oscillations of sonorous bodies, and discloses to us secret analogies between phenomena—analogies which apparently must for ever have escaped our experience. This science is, in a manner, destined to aid our instruments and our senses; it brings the study of nature to a limited number of primordial observations, which have for their object the measuring of the dimensions or the specific qualities of bodies."

(To be continued.)

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## ARTICLE XI.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Volatility of Bismuth.*

M. Chaudet has made a set of experiments upon the fixity of bismuth, from which it appears, that this metal, though covered with charcoal, is completely volatilized if it be kept for a sufficient time at a temperature of about 30° Wedgewood.—(Ann. de Chim. et de Phys. ix. 397.)

#### II. *On the Alloy of Platinum and Lead.* By Dr. Clarke.

(To Dr. Thomson.)

DEAR SIR, *Elsenham Hall, near Bishop's Stortford, Essex, July 10, 1819.*

One of your correspondents mentions the astonishing affinity of *platinum* and *lead* at no very exalted temperature before the common blow-pipe. I think this deserves more of your notice than perhaps you have hitherto shown it. The experiment with these metals is one of the most striking you can conceive; but you must make it cautiously, or you will be liable to have your hands very much burned. If you take two pieces of lead-foil and platinum-foil of equal dimensions, and roll them together, and place the roll upon charcoal, and direct the flame of a candle cautiously towards the edges of the roll, at about a red



heat, the two metals will combine with a sort of explosive force, scattering their melted particles off the charcoal, and emitting light and heat in a very surprising manner. Then there will remain upon the charcoal a film of *glass*; which, by further urging the flame towards it, will melt into a highly transparent globule of a sapphire blue colour. Also if the *platinum* and *lead* be placed beside each other, as soon as the platinum becomes heated, you will observe a beautiful play of blue light upon the surface of the lead, becoming highly iridescent before it melts.

Very truly yours,

E. D. CLARKE.

### III. Heat produced by the Gas Blow-pipe.

Dr. Clarke lately in his lecture room at Cambridge, in presence of the students, kept above half an ounce of platinum in a boiling state before the gas blow-pipe. The mass when cool was publicly weighed.

### IV. Magnetic Iron Ore.

A fact respecting the magnetic iron ore of Succasunty, belonging to Governor Dickerson of New Jersey, is stated by Col. Gibbs in *Silliman's Journal of Science* (i. 89), and deserves, I think, to be generally known. The following is Col. Gibbs's own statement.

"The proprietor, a gentleman of distinguished science, informed me of a singular circumstance attending it, which was too important to be left unnoticed. The mine is wrought to the depth of 100 feet; direction of the bed, north-east and south-west; inclination nearly perpendicular. The ore in the upper part of the bed is magnetic, and has polarity; but that raised from the bottom has no magnetism at first, but acquires it after it has been some time exposed to the influence of the atmosphere."

### V. On Gauze Veils. By Mr. Murray.

(To Dr. Thomson.)

SIR,

London, June 24, 1819.

We are very much obliged to your ingenious correspondent Mr. Bartlett, for his suggestion respecting gauze veils considered as preventives of contagion. Three winters ago I found great relief from using a piece of black crape before my eyes, in a snow storm, and I pointed out this circumstance in the *Philosophical Magazine*, adverting at the same time to its importance to the mariner in such a peril. Bell, in his Embassy to Ispahan, found that the natives of the country through which he passed were in the habit of using hair cloth to prevent their eyes being injured by the reflective powers of the snow.

A handkerchief held before the mouth, &c. in the fogs which obtain in this metropolis certainly prevents the unpleasant sensations which we otherwise experience from their effects.

In going to Tivoli, I used a similar guard with advantage when passing the brook flowing from the "Lago di Tartaro," the odour of which is almost insupportable.

The use of crape, &c. is evidently to guard the eyes from the pain of reflected light, and the merits of wire gauze in the safety lamp for miners depend on its cooling powers. The gauze veil in the case suggested acting as a screen or filter, would oppose the passage of humid air, and with it the elements of contagion, whatever they may be. I found that breathing on the rings of Breguet's metallic thermometer moved the index to the left from zero (marked "tempéré") to  $15^{\circ} +$ ; but when I repeated the experiment through a silk screen, it scarcely reached  $12^{\circ}$ .

I have the honour to be, Sir,

Your most humble and very obedient servant,

J. MURRAY.

#### VI. *Evolution of Carburetted Hydrogen Gas from Coal.*

By Mr. Murray.

Mr. Longmire ascribes the formation of carburetted hydrogen in mines to the high pressure under which coal was formed; and Sir H. Davy reiterates the same opinion. This assumption, however, is evidently hypothetical. Mr. Hodgson has clearly proved that when coal is *broken* under water, carburetted hydrogen is disengaged. Now it is a well known fact, that this gas obtains in greatest abundance in the vicinity of *dykes which abrupt the coal*. It appears to me, therefore, very evident, that these dykes by dislocation of the strata and crumbling the coal, for we know that this is palpably the fact in coal connected with *faults*, are the effective cause of disengaging the fire-damp.

#### VII. *Pus of Venereal Sores.*

M. Chevallier has published the result of a set of experiments to determine the constituents of the pus obtained from venereal sores. We are afraid that such experiments are not likely to lead to any consequences of much importance. Pus, in all probability, is of the same nature when laudable, whatever has occasioned the abscess from which it was extracted; but when the ulcers are ill-conditioned, the appearance of the pus is much altered, and its constitution in all probability changed. It would be a material point to determine the nature of this alteration. I suspect from the facts which have been recorded by observing surgeons, that in certain cases ill-conditioned pus is alkaline; while in others it is acid. The pus from venereal sores, especially when the constitution has been broken down by the excessive use of mercury, is frequently ill-conditioned, and it seems to be always more or less alkaline, and the alkali is always ammonia. Its smell is often fetid, and one would be tempted to suspect that it has undergone a kind of putrefaction.

Some of the ulcers from which the pus subjected by Cheval-



organs which leads to the formation of such unnatural substances. These concretions are commonly discharged enveloped in mucus during a violent fit of coughing, and sometimes accompanied by blood, but frequently not. Indeed hæmoptysis and its consequences appear to constitute the great danger attending them, as, from the violence of the irritation they produce, such an occurrence is not unlikely to happen in plethoric or otherwise pre-disposed individuals.

The following case will illustrate the points in question. A gentleman about 30 years of age was sent to me by a friend a few weeks ago, who had for several years occasionally coughed up these concretions. His general health appeared to be good, and he assured me that he was not aware that he had any disease of his lungs whatever. They were discharged as usual with violent coughing, but without blood. They varied in size from that of a common pin's head to twice or thrice that magnitude, and their shape and surface were irregular and different. On being subjected to analysis, they were found to consist chiefly of phosphate of lime with some carbonate of lime, and a cementing animal matter, which retained the size and shape of the concretion after the earthy matter had been removed by an acid. Pulmonary concretions have been examined by different chemists. Some have stated their composition to be as above, while others have found them to consist entirely of phosphate or carbonate of lime united to an animal matter. I have never seen an example of either of these instances, and strongly suspect that those chemists who have asserted them to consist occasionally solely of carbonate of lime have suffered themselves to be mistaken.

IX. *Earthy Mass discharged from a Wen.* By Dr. Prout.

This bony mass was discharged by ulceration and suppuration from a wen situated in the back part of a man's neck. When first separated, it was exceedingly fetid and heavy, but diminished in feter and weight as it lost its moisture. Its general shape was oval, but it was flattened and irregular on the side which appears to have been that next the body. The length of its greatest diameter was  $2\frac{1}{8}$  inches, of its lesser diameter  $1\frac{1}{4}$  inch, or when taken perpendicularly to the flattened side, only one inch. It weighed, when tolerably dry, 580 grains. In colour and general appearance, it resembled bone; but its internal structure was different, as it seemed to be made up of ill-defined granular masses, the interstices of which were filled with a less compact substance. Hence it could be easily broken.

When exposed to heat, it burned with a flame as if it contained an oily matter, and after being burnt, it still retained its shape, though it was very fragile. A small fragment submitted to analysis gave the following as its constituents, the proportions of which, however, from the minute quantity operated on, are only to be considered as approximations.





person might contrive two valves to prevent the mixture of the gases from being driven back, and the passage of the flame, which would, in my opinion, preclude all possibility of explosion.\*

H. B. LÆSON.

### XI. *Larch Tree (Pinus Larix).*

The first larch trees ever seen in Scotland were sent to the Duke of Athol at Dunkeld, in the year 1738, in two garden-pots. They came from Switzerland, and were at first put into the green-house. By degrees, it was discovered that they could bear the winter in Scotland without injury. They were, therefore, planted in the Duke of Athol's park at Dunkeld, very near his house. There they may be still seen, having grown in the course of 81 years, which have elapsed since they were planted, to the size of very large trees. Their circumference, about a foot above the ground, is nearly 18 feet; and at the height of eight feet, the circumference is nearly 14 feet. Thus in 81 years they have produced as much wood as an oak would in the course of several centuries. From these two parent trees have sprung all the larches which abound so much in Scotland.

The larch tree is now almost every where preferred to the Scotch fir, which it has in a great measure superseded. It is a much more beautiful tree; it vegetates much more rapidly; is not so difficult to please in a soil; and is at least as hardy, if not more so. The larch wood is not inferior to that of the fir, and the bark is purchased by the tanner for about half the price that he pays for oak-bark. Trials have been made of it for ship-building, which have answered very well. At present, there is a cutter building of it at Perth.

### XII. *Wood in Scotland.*

The reproaches which Dr. Johnson in his Journey to the Hebrides threw out against Scotland for its want of wood, though perhaps a little exaggerated, were probably not very far from the truth. That country, about a century before, had been covered with old wood; which, being considered by the proprietors as of no value, was allowed to fall into decay without any effort to preserve it; while the introduction of sheep effectually prevented the growth of young wood. Accordingly when the old trees fell down from age, the country became quite bare. But the reproaches of Dr. Johnson turned the attention of the Scottish landlords to planting; and in many parts of Scotland, particularly in Perthshire, the defect of which Dr. Johnson complained has been completely removed. The two greatest planters of trees in that county, and perhaps in Scotland, are the Duke of Athol and the Earl of Breadalbane. Each of these noblemen, I have been informed, has planted at least 60,000,000 of trees.

\* By the direction of the tubes, c c, the gases will cross one another, and thereby be more completely mixed.





## Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
July								
1	Morn....	29.366	56°	46°	WNW		Fine	47½
	Noon....	29.387	59	38	WNW		Cloudy	61
	Even....	29.400	55	42	WNW		Fine	48½
2	Morn....	29.423	58	50	SW		Fine	62
	Noon....	29.395	60	52	SW		Rain	56½
	Even....	29.342	57	90	SW		Rain	71
3	Morn....	29.352	61	66	WSW		Cloudy	57
	Noon....	29.362	69	45	SW		Fine	79½
	Even....	29.387	55	56	SSW		Cloudy	61
4	Morn....	29.346	69	53	SE by S		Cloudy	78
	Noon....	29.316	78	41	Var.		Thunder	61
	Even....	29.400	70	43	SW by S		Fine	73
5	Morn....	29.403	68	55	WSW		Fine	60
	Noon....	29.422	77	43	Var.		Thunder	67
	Even....	29.416	69	56	N by W		Rain	57
6	Morn....	29.504	62	65	NE		Cloudy	58½
	Noon....	29.498	66	55	ENE		Showery	67½
	Even....	29.503	60	73	NNW		Rain	55½
7	Morn....	29.700	60	68	NW by N		Cloudy	69
	Noon....	29.725	69	47	NE		Cloudy	50
	Even....	29.715	66	49	NE		Very fine	68½
8	Morn....	29.585	60	73	N by W		Rain	53½
	Noon....	29.557	65	57	N by W		Cloudy	72½
	Even....	29.543	64	53	W by S		Cloudy	57½
9	Morn....	29.624	58	61	WSW		Cloudy	68½
	Noon....	29.658	65	45	WSW		Fine	51
	Even....	29.718	61	44	WNW		Fine	67
10	Morn....	29.668	59	50	W		Fine	54
	Noon....	—	—	—	—		—	67
	Even....	29.628	60	50	WNW		Cloudy	52½
11	Morn....	29.634	60	55	NW by N		Cloudy	68½
	Noon....	29.642	65	47	NW		Cloudy	55
	Even....	—	—	—	—		—	72½
12	Morn....	29.636	66	54	NW		Fine	57½
	Noon....	29.665	71	51	NW		Cloudy	68½
	Even....	29.725	65	50	NE		Fine	51
13	Morn....	29.741	63	53	NNE		Cloudy	67
	Noon....	29.740	68	46	NNE		Cloudy	54
	Even....	29.722	61	53	NNE		Cloudy	67
14	Morn....	29.721	59	48	NE by N		Fine	52½
	Noon....	29.700	66	44	NE		Cloudy	68½
	Even....	29.640	60	53	NE		Fine	55
15	Morn....	29.605	56	58	NE		Sm. rain	70½
	Noon....	29.581	60	50	NE		Fine	56½
	Even....	29.566	58	53	NNE		Very fine	72½
16	Morn....	29.567	56	55	NE		Cloudy	55
	Noon....	29.567	66	57	N by E		Cloudy	70½
	Even....	29.567	62	53	NNE		Fine	56½
17	Morn....	29.560	63	51	WNW		Cloudy	58½
	Noon....	29.580	68	45	WNW		Cloudy	72½
	Even....	29.600	65	53	Calm		Cloudy	58½
18	Morn....	29.568	65	57	SW by W		Cloudy	72½
	Noon....	29.500	71	42	SW by W		Fine	—
	Even....	29.434	64	53	SSW		Fine	—



## ARTICLE XIII.

## METEOROLOGICAL TABLE.

1819.	Wind.		BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
			Max.	Min.	Max.	Min.			
7th Mo.									
July	1	N W	29.92	29.88	65	47	—		59
	2	S W	29.92	29.87	70	58	—	2	62
	3	S W	29.87	29.82	80	54	36		67
	4	Var.	29.85	29.82	86	59	—	8	63
	5	N W	29.96	29.85	85	58	29		65
	6	N W	30.18	29.96	74	56	—	52	65
	7	N W	30.18	30.06	81	55	—	7	67
	8	N W	30.12	30.01	69	53	—	3	78
	9	N W	30.17	30.15	73	48	—		67
	10	N W	30.15	30.12	73	51	44		61
	11	N W	30.12	30.12	70	60	—		63
	12	N W	30.18	30.12	75	57	—		63
	13	N E	30.18	30.18	72	46	—		63
	14	N E	30.18	30.09	74	53	40		65
	15	N E	30.09	30.08	72	52	—		67
	16	N E	30.08	30.07	78	46	—		64
	17	N W	30.07	30.04	77	57	29	—	60
	18	W	30.04	29.72	77	56	—		63
	19	S W	29.72	29.29	78	59	—	23	61
	20	S W	29.45	29.27	81	51	—	50	67
	21	N W	29.96	29.45	64	47	50		70
	22	N W	30.16	29.96	70	42	—		60
	23	N N W	30.21	30.16	82	50	—		58
	24	N N W	30.21	30.13	85	52	40		58
	25	S E	30.13	30.07	79	47	—		54
	26	N E	30.17	30.13	79	47	—		57
	27	N	30.20	30.17	77	56	43		65
	28	N E	30.19	30.17	78	52	—		65
	29	N E	30.17	30.13	81	53	—		65
	30	N E	30.13	30.08	84	57	54		57
	31	N E	30.05	30.02	85	56	15		57
			30.21	29.27	86	42	3.80	1.45	63

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.



# ANNALS

OF

## PHILOSOPHY.

OCTOBER, 1819.

### ARTICLE I.

*On the Oxides and Salts of Mercury.* By Mr. Donovan.

#### PART I.

THE combinations of mercury, whether regarded as having been the cause of a revolution in medicine or in chemistry, are entitled to the highest consideration. Indeed their value seems to have been duly appreciated. The number of inquirers is considerable, but the results of their investigations are so different, that the subject is still involved in perplexity.

Chemists have differed as to the number of mercurial oxides, as well as to the ratio of their elements. It is the principal design of the present inquiry, first, to ascertain the constitution of the two oxides that have been universally admitted; and next to discover whether or not there be any others.

The diversity of opinion with regard to the ratio of oxygen in the oxides will appear by the following table :

	One hundred parts of				One hundred parts of metal take up of oxygen,	
	Black oxide consist of		Red oxide consist of		Black oxide.	Red oxide.
	Metal.	Oxygen.	Metal.	Oxygen.		
Doerhaave.....	—	—	90.73	9.27	—	10.00
Kirwan.....	—	—	92.60	7.40	—	8.00
Lavoisier.....	—	—	93.00	7.00	—	7.52
Chaptal.....	—	—	90.00	10.00	—	11.10
Bergman.....	96.15	3.00	—	—	3.10	—
Wenzel.....	97.60	2.30	—	—	2.35	—
Fourcroy, and Thenard	96.16	3.84	92.60	7.40	4.00	8.00
Davy.....	96.20	3.80	92.70	7.30	3.94	7.88
Chenevix.....	89.30	10.70	85.00	15.00	11.98	17.60
Zaborda.....	95.00	5.00	90.00	10.00	5.26	11.10
Braamcamp and Si- queira Oliva.....	92.50	7.50	90.00	10.00	8.10	11.10
Sefstrom.....	96.20	3.80	92.68	7.32	3.95	7.90





boiling alkaline solution on calomel, the filtered solution on cooling let fall a yellow oxide. These were the circumstances of the three different results.

5. From a variety of considerations, to be detailed hereafter, it appeared that the black powder is the real oxide contained in calomel; and hence as the green oxide is a mixture of the black and yellow, the additional dose of oxygen must be derived from another portion of the black. But as there is no known intermediate degree between the black oxide and the metal, I concluded that a portion must have been reduced to the metallic state. On submitting some of the powder to the microscope, I observed innumerable minute globules; and these could be collected into one by triturating the powder, when perfectly dry, in a mortar. By the same management both the grey and black powder (4) afforded metallic mercury. In many cases, the quantity of mercury was alike: the average was about 25 per cent.

6. The experiments were repeated in various ways on the muriate, nitrate, sulphate, and acetate of mercury, the metal being in the first degree of oxidation in each. With these salts, the solutions of potash, soda, ammonia, and lime, were used: in all cases the mercury was in part reduced to the metallic state; and in no case could I obtain a pure black oxide.

7. Hence the whole of the preceding amounts to this, that pure alkaline substances not only deprive the protosalts of mercury of their acid, but also their bases of a part of their oxygen; and this portion of oxygen, in some cases, unites with the remaining portion of the oxide, and when the quantity is very small passes into new combinations. What these combinations are I have not been able to determine. It is easy to conceive that the minute quantity of oxygen belonging to a few grains of metal might elude detection. It appears, therefore, that the oxide as usually obtained is not the protoxide, but contains either peroxide, or metal, or both; and hence that the analysis given of this substance must want precision.

8. To ascertain whether alkalies decompose the oxide contained in the oxygenized salts of mercury, I dissolved different portions of pure red oxide in dilute nitric, sulphuric, and muriatic acids. The oxide precipitated from each by a large quantity of potash was found to be unchanged.

## II. *Analysis of the Protoxide and Peroxide of Mercury.*

9. From the last section, it appears that pure black oxide of mercury is not obtained by the usual process of decomposition: either the resulting oxide will contain metallic mercury, or part will be brought to the state of peroxide; while mercury is reduced; and as this portion, on account of its specific gravity, occupies the lowest stratum, there is hence an unequal distribu-

tion of the oxygen, although the absolute quantity may be the same. In the former case, the ratio of oxygen affords analysis would be too small; in the latter, unless the product of the decomposition were employed, the ratio of oxygen would be too large.

10. By long continued agitation, in Boerhaave's mortar, mercury is partly oxidized, but a great portion is only divided. From such a powder as this, Dr. Priestly obtained a large quantity of metallic mercury. Beside that, if the oxide be pure, agitation has but little effect in oxidating it. It is therefore, a bad mode of forming this oxide.

11. I endeavoured to obtain pure black oxide by triturating mercury with syrup; but after continued trituration of the metal for 40 hours, I obtained 54 gr. of unaltered mercury, hence there were but 6 gr. oxidized.

12. After various attempts to procure black oxide for analysis, I was obliged to proceed in the following manner. About six grains of calomel were triturated well with water, and a quantity of solution of pure potash was dropped once over it during brisk trituration. This process was repeated several times, with new portions of calomel, until a sufficient quantity of oxide was obtained. This powder did not contain any of red oxide. It was extremely well dried in the shade and triturated until it would give out no more globules of mercury. There might be, perhaps, a few grains of metal still contained, but the quantity must have been too small to influence the result of the analysis. The globules were easily collected in water and separated.

13. Fifty grains of this oxide were introduced into a glass tube, 10 inches long, and  $\frac{1}{4}$  diameter, sealed at one end. The part of the tube which contained the oxide was gently heated to redness in a charcoal fire. A quantity of oxide sublimed, but a portion of it at that high temperature combined with the oxygen present, and formed red oxide. By means of a polished iron scraper nicely adapted to the cylindrical tube, I scraped down to the bottom every thing that had sublimed, and introduced a capillary tube, by means of which the large tube was filled with hydrogen. The end of the large tube was again heated to redness, and when cold, the same process was repeated with the scraper, and the tube again filled with hydrogen. The third heating had reduced the oxide considerably, but a white powder remained which was not metallic mercury, probably abraded from the mortar during the trituration. The oxide employed in the analysis, I found, by a previous experiment, to contain water. The result of the analysis is as follows: Mercury, 47.48; oxygen, 1.96; water, 0.31; residue, 0.25. Therefore, deducting the two latter ingredients from the analysis of the black oxide will be

Mercury. ....	96.04
Oxygen. ....	3.96
	<hr/>
	100.00

14. To discover the composition of the red oxide, I made use of that prepared by heat alone, commonly called calcined mercury: 50 grains of this were introduced into a green glass tube, like the former, and the end of the tube was gradually heated to redness, and maintained so until the decomposition was complete. There was no trace of water, the mercury came over easily, and was quite bright, and there was no need of filling the tube with hydrogen, or of a second heating. The mercury, when collected, weighed  $\frac{16}{3}$  gr.; there were minute grains of some foreign body weighing  $\frac{3}{4}$  gr. which, being deducted from the original oxide, gives its analysis as follows:

Mercury. ....	92.75
Oxygen. ....	7.25
	<hr/>
	100.00

15. From these analyses, it would appear that 100 parts of mercury take up to form black oxide, 4.12; and to form red oxide, 7.82. My estimate of the red oxide agrees very nearly with the analyses of Kirwan, Lavoisier, Fourcroy, Thenard, Davy, and Sefstrom; but the four latter rate the oxygen in the black oxide lower, it being just half of what enters into the red. These philosophers, however, used a black oxide which contained a metallic mercury, and I separated that portion from mine: hence the difference, probably. My analysis of the black oxide, being not exactly coincident with the doctrine of definite proportions, may be thought inaccurate. The difference, however, is not great: if the red consist of 7.82 for every 100 of mercury, the black would consist of 3.91; whereas I rate it at 4.12—a difference of about  $\frac{1}{3}$ th of a grain; and notwithstanding the smallness of this discordance, I could not, although the experiments were often repeated, bring out any other result.

### III. *On certain Properties of the Mercurial Oxides.*

Previously to entering on the constitution of the salts of mercury, it will be expedient to state certain properties of the oxides, a knowledge of which is necessary to avoid errors that otherwise we might fall into with regard to their nature.

16. When the black oxide of mercury is exposed to heat, it is known to become yellow. The circumstances of this change I find to be as follow. An additional dose of oxygen is absorbed, but it is not from the atmosphere, as is commonly supposed. Part of the oxide is reduced, the mercury is volatilized, or remains, if the heat be low; while the oxygen liberated imme-



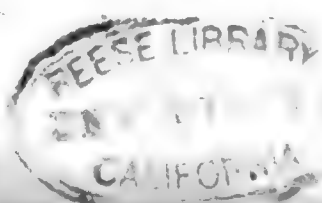
diate between the black and red. Dr. Thomson is of this latter opinion. He supposes this deutoxide to be identical with that estimated by Chenevix at 89.3 mercury, and 10.7 oxygen, and the same as the yellow powder which appears during the calcination of mercury, the mercury first becoming black, next yellow, and lastly red. Other chemists conceive that if heat be applied during the solution of the mercury, the metal unites to several doses of oxygen, according to the degree of heat, the ratio of mercury to the acid, and the length of time; and that several nitrates may be thus formed, which to alkalies afford precipitates of different colours. The writer of the article "Mercury" in Rees's Cyclopædia states, that when lime-water is added to solution of nitrate of mercury made in the cold, a yellow precipitate appears, which is an oxide distinct from the black and the red. He modifies his calculations accordingly, and describes some of the combinations of this oxide.

22. With a view of coming to some determination with regard to these very different statements, I made cold solutions of mercury in nitrous acid of various degrees of concentration. In some cases, the acid was little stronger than barely to act on the metal; the latter was added by grains at distinct intervals, and the vessel was kept immersed in cold water. Each of these solutions being decomposed by muriate of soda, the filtered liquor gave with potash a copious deposition of peroxide. I satisfied myself that this was not owing to any partition of oxygen during the precipitation, for a nitrate prepared by dissolving black oxide in dilute acid, and precipitated by salt, afforded no peroxide to potash. The numerous trials I made led me to the following general conclusion.

When nitrous acid of any effective strength is made to act on mercury in the cold, the metal is partly oxidized to the *minimum*, and partly to the *maximum*; the greater the ratio of acid, the greater will be the quantity of peroxide produced. There are, therefore, two nitrates produced together.

That this solution contains none other but these two salts appears from the following. If the solution, not too acid, be concentrated by evaporation, or without evaporation if the original acid had been about specific gravity 1.280, a quantity of crystals will be formed, which are the real nitrate containing the black oxide, and no other. If the mother liquor be further evaporated, more of the same salt will be obtained; but when all the nitrate has crystallized, the remaining liquor will be found to contain the peroxide with only the least traces of the black; and when evaporated to dryness, a white salt will result in every respect resembling that prepared by solution of peroxide in nitric acid. Hence the original solution was a mixture of nitrate and oxynitrate, but not a nitrate of a medium oxide.

23. The formation of the nitrate in the solution has been supposed to be limited by two circumstances; first, that the solution







This is the true subnitrate ; it is yellow when it contains more acid, and white when nearly saturated.

30. When a few drops of an alkaline or earthy solution are mixed with solution of nitrate of mercury, a white powder appears ; if more alkali be added, the white powder becomes grey ; at length dark grey : this is the same subnitrate. Further additions of the alkali take up the whole of the acid, and the oxide is left according to the conditions already detailed (4).

31. When red oxide of mercury is dissolved in dilute nitric acid, and evaporated to dryness, a white crystalline mass is obtained, which deliquesces, although not to perfect fluidity. This is oxynitrate of mercury.

The same salt is formed by boiling crystals of nitrate of mercury in nitric acid, an effervescence being excited owing to the abstraction of oxygen, or by dissolving mercury in a large quantity of nitric acid, and evaporating to dryness. This salt, in a certain quantity, is formed in every case where mercury and nitric acid, of whatever strength, are suffered to act on each other, and remains dissolved after all the nitrate has been separated by crystallization.

Cold water, unless acidulated, decomposes the oxynitrate into an acidulous soluble portion, and an insoluble brown powder. A stream of sulphuretted hydrogen, passed through this powder, separates nitrous acid, so that this is suboxynitrate. By means of boiling water, Thenard separated the whole of the nitrous acid.

32. Under ordinary circumstances, if nitric acid be boiled on mercury, and the solution evaporated to dryness, a yellow mass is produced, which has been supposed to be oxynitrate ; and the yellow powder which may be obtained from it by means of cold water has been stated as suboxynitrate. But this is not so, for the yellow mass is a mixture of nitrate and oxynitrate with their subsalts. When cold water is added, the nitrate affords whitish-yellow subnitrate, the oxynitrate gives reddish-brown suboxynitrate, and the mixture of these produces the yellow powder which has been mistaken for nitrous turbith. If the water be boiling, the subnitrate is still further deprived of its acid, and becomes the blue-grey subsalt, the intermixture of which with the rest gives the greenish tinge. The real nitrous turbith is red-brown.

33. It is now necessary to ascertain the constitution of the oxides that form salts by combination with nitric acid. The nitrate and subnitrate evidently contain the same oxide, and when this is separated by means of potash, its analysis is that which has been already stated (13). The same thing is shown synthetically ; for when black oxide is presented to nitric acid, either of these salts is produced according to the dilution, and no gas is evolved.

That the oxynitrate and suboxynitrate contain the same oxide,



38. The analysis of this oxide is stated by Payse at 18 per cent of oxygen. Chaptal obtained so much as 20 per cent. while what is considered peroxide contains but 7 or 8. From this, red precipitate would appear to be a distinct oxide, and that in the highest degree of oxidation.

To ascertain this point, I heated 50 gr. of red precipitate for 10 minutes in a green glass tube to about  $350^{\circ}$ , and obtained  $\frac{1}{32}$  of water, which I afterwards found to be hygrometric. The analysis was then conducted in the manner already detailed (13); the reduced mercury being scraped down, the tube filled with hydrogen, and the heating repeated, for all this was necessary, although it was not in the case of calcined mercury. In this manner I was surprised to find that the analysis of this exactly corresponded with that of calcined mercury, the oxygen amounting to but 7.2 per cent. instead of 18 or 20. Perhaps Paysse and Chaptal examined a red precipitate which contained some undecomposed nitrate of mercury.

Hence it appears that red precipitate is precisely the same as the red oxide prepared by calcination; and it is no objection to urge the poisonous and escharotic effects attributed to the former; they apply equally to both, or more properly to neither. John de Vigo used red precipitate internally in the dose of three or four grains as a remedy against the plague. Mathiolus, almost three centuries ago, used it in the cure of the venereal disease in the dose of five grains, but declares it dangerous unless *previously well washed* in plantain water. Lemery himself gave it in doses of three or four grains. For my own part I ventured, as an experiment, to take it in the dose of one grain at intervals of two days, but well levigated, and I could find no effect of any kind from it. When it produces violent effects, it perhaps has not been well levigated, or contains undecomposed oxynitrate.

From all the foregoing observations on the combinations of nitric acid and mercury, it appears that none of these salts afford any other oxide than the black and the red. We shall next proceed to the consideration of the sulphuric acid.

(To be continued.)

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## ARTICLE II.

*On Cyder Making, with Queries.* By the Rev. J. Venables.

(To Dr. Thomson.)

SIR,

As I have always considered the scientific journals of the present day as a medium through which not only the learned may communicate the result of their researches to the public;



immature apples of a country which, compared with others, enjoys so little of the sun, the quantity of malic acid must evidently be excessive. In what way this may be best diminished and a part of it neutralized, I trust some of your chemical friends will be so obliging as to inform me.

2. We are told by chemists that a certain portion of vegetable mucilage is necessary to a perfect fermentation; but if the quantity be too great, the fermentation also will be excessive, and convert the sugar not into spirit, but into acid. The great desideratum, therefore, in cyder making, I consider to be some proper method of fining the liquor before the fermentation begins, some process to bring it to make an early deposit of the vegetable mucilage or pulpy matter, which makes it so muddy upon leaving the press. If the juices could be extracted from the apple in a more pure and unmixed state, the fermentation would be moderate, and almost imperceptible, and the sweetness, richness, and flavour of the cyder preserved. But when the fruit has been well ground in the mill, and is afterwards submitted to the press, it seems impossible to prevent a superabundant quantity of vegetable matter from being expressed with the juices. It is this which causes the excessive fermentation at first, and when set afloat afterwards by the least agitation of the cask, or even change of weather, continues the fermenting process till the whole of the saccharine principle is converted into acid, and the cyder changed into that harsh and unpleasant beverage of which we have so often reason to complain.

Much of the pulpy deposit may be separated from the liquor by racking it daily before the fermentation begins, and, perhaps, the commencement of the fermentation may be delayed, and sufficient time obtained for the complete accomplishment of the object in view, if the deficiency occasioned by racking were made up by the frequent addition of old cyder of the preceding year. To rack cyder, as it is commonly done, in its turbid state, during the violence of the fermentation, when every particle of previous sediment is again set in motion, I consider perfectly useless, if not injurious and absurd. But for the best method of precipitating the pulpy matter, and obtaining an immediate deposit of those impurities, which are hurtful to the liquor, I must again solicit the advice of the practical and experienced chemist.

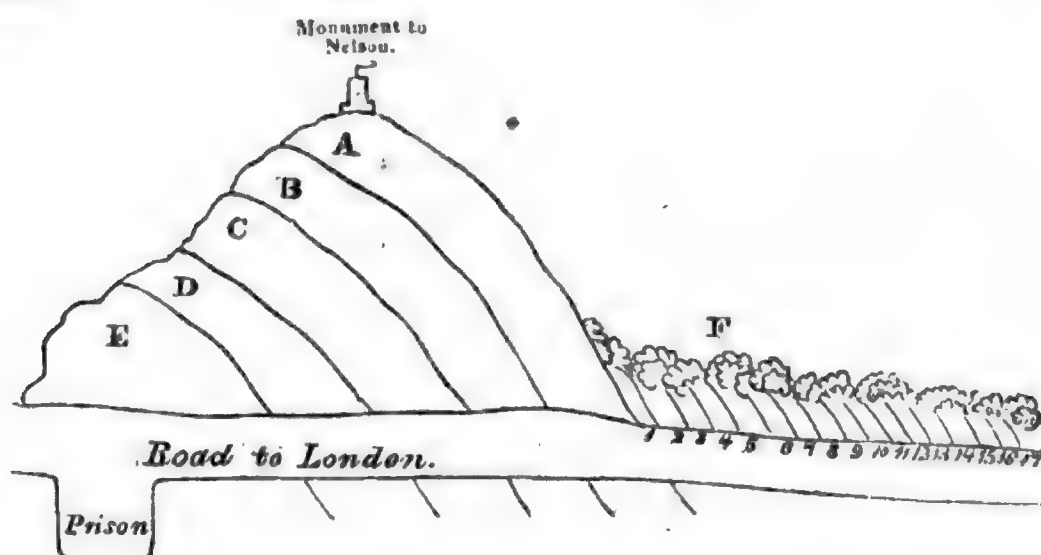
3. During the process of fermentation, a great quantity of carbonic acid gas is evolved; but the moment the fermentation ceases, the cyder begins to absorb oxygen from the atmosphere, and if freely exposed after it has ceased to ferment would quickly turn sour. In order, therefore, to permit the carbonic acid gas to escape, and to prevent the absorption of oxygen, which is the principle of acidity, I have generally filled the casks nearly full, and closed them with bungs fitted with safety valves. But here also I would defer to the superior judgment of the chemist,





are connected with the newer rocks of transition. From the coal field rise in many places beds of greenstone, in general forming small conical and round-backed hills. Other eminences are composed of amygdaloid, claystone, and other porphyries; and basalt and trap tuff occur in an overlying position. Of these, it is not my intention to speak otherwise than as conveying a general idea of the geological relation of the wacke above referred to.

The structure of Calton Hill has been exposed by the recent improvements, and in particular by a section made in the construction of the new road to London. The rock occurring in greatest abundance, and which is probably the fundamental bed, is a porphyry, the basis of which in general is claystone, which in many places passes into felspar, in others becomes a distinct greenstone. Numerous veins of calcareous spar traverse it in different directions; and I am lately informed, that very beautiful examples of veins of greenstone of contemporaneous formation with the rock itself, have been discovered *in* the greenstone. Upon the porphyry rests a bed of trap tuff, upon this other beds of the two rocks repose, that at the summit being porphyry. The back of the hill (as we pass from the city) is a spot of peculiar interest, consisting of alternate thin beds of bituminous shale, sandstone, wacke, and clay ironstone, disposed in a manner which will be best understood by a rough outline taken on the spot.



A. Porphyry.

B. Trap tuff.

C. Porphyry.

D. Trap tuff.

E. Porphyry.

F. Beds of wacke, &c. upper part concealed by vegetation.

1. Bituminous shale.

2. Wacke.

3. Sandstone.

4. Bituminous shale, with clay ironstone.

5. Wacke.

6. Bituminous shale

7. Wacke, *with calc. spar.*

8. Bituminous shale.

9. Wacke.

10. Bituminous shale passing on both sides into

11. Wacke, and *calc. spar.*



## ARTICLE IV.

*Researches on a new Mineral Body found in the Sulphur extracted from Pyrites at Fahlun.* By J. Berzelius.

(Continued from p. 166.)

6. *Seleniuret of Copper.*—If we precipitate sulphate of copper by seleniuretted hydrogen gas, we obtain a seleniuret of copper in black flocks, which, when dry, become of a dark-grey colour, and may be polished by a hematite. Exposed to a red heat in a distilling apparatus, this seleniuret gives out one-half of its selenium, and leaves a melted button of protoseleniuret of copper. This last seleniuret is easily formed with the evolution of heat when copper and selenium are heated together. The compound becomes liquid long before it is heated to redness, and gives a steel-coloured button with a compact fracture, very like grey sulphuret of copper. When exposed to the naked fire, it first loses a certain quantity of selenium, after which it undergoes no further alteration, and after being long roasted, it still yields a mass more fusible than copper, which breaks under the blow of a hammer, and exhibits a fracture of a grey colour.

7. *Seleniuret of Lead.*—Selenium and lead unite with the production of heat. The lead swells, and gives a porous mass of a grey colour, which does not melt at a red heat, which easily receives impressions, admits of being polished, and then assumes a silver-white colour. When heated, seleniuret of lead gives out first a little selenium, and then evaporates in a white smoke. The residue gives at a high temperature marks of a commencing fusion. Seleniuret of lead exposed to the flame of a blow-pipe loses a part of its selenium, becomes oxidized, and produces a subseleniate of lead, which afterwards suddenly penetrates the charcoal, is decomposed, and leaves upon the surface of the coal a white pellicle of seleniuret of lead reduced. Lead at a high temperature is capable of uniting with a little seleniuret of lead. By this union, it becomes whiter, and less fusible.

8. *Seleniuret of Silver.*—Silver is blackened by the fumes of selenium. If we heat the metal with an excess of selenium, heat is disengaged, and a very fusible mass is produced, from which the excess of selenium may be separated by distillation. The compound has a grey colour, and while it remains liquid, its surface is brilliant, and polished like a mirror. This seleniuret is fusible at a temperature greatly below that of a red heat. When cold, it is grey, somewhat ductile, and may be flattened a little before it breaks. When heated before the blow-pipe, it loses a portion of its selenium, and with it a good deal of its fusibility. In a strong fire, kept up for some time, it continues liquid; but very little of the selenium is disengaged. The sele-



other portion of that acid dissolves with the corrosive sublimate. This phenomenon sometimes took place when selenium was extracted from red sulphur, and embarrassed me at first a good deal. I could perceive no reason for the reduction of the selenium by the addition of muriatic acid, especially after ascertaining that no chlorine was formed. I found at last that it was owing to the formation of a little protoseleniate of mercury, which precipitates along with the sulphur not dissolved.

Seleniuret of mercury dissolves rapidly in nitromuriatic acid, even without the assistance of heat.

10. *Seleniuret of Bismuth.*—This combination takes place with the evolution of a little heat. It melts at a red heat, and then has a polished surface. When cold, it has the metallic lustre, a silvery colour, and its fracture is crystalline.

11. Gold and selenium do not combine when heated together; but I have no doubt that a compound of them may be obtained by precipitating a solution of gold by seleniuretted hydrogen gas.

12. *Seleniuret of Palladium.*—Palladium combines readily with selenium, and heat is evolved. The compound is grey, coheres together, but does not melt. Before the blow-pipe selenium is disengaged, and when the heat is strong, the seleniuret melts into a greyish-white metallic button, brittle, and having a crystalline fracture; it still, therefore, contains selenium.

13. *Seleniuret of Platinum.*—Selenium combines very readily with platinum in powder, occasioning the evolution of a strong heat. The compound is a grey powder, not fused. In the fire, the selenium is easily oxidized, and leaves the platinum pure. Platinum crucibles are easily attacked by the seleniates when they are heated to redness in these vessels, or when dry seleniate of ammonia is evaporated in them. The surface of the metal acquires a brownish-grey colour, and cannot be cleaned; but the selenium is volatilized if the crucible be heated to redness without being covered.

14. *Seleniuret of Antimony.*—These two bodies unite easily and with the production of heat. The compound melts into a metallic button, having a crystalline fracture. If it be strongly heated in the air, it becomes covered with a vitreous scoria.

15. *Seleniuret of Antimony with Oxide of Antimony.*—These two substances easily unite, and form a yellowish-brown mass, transparent when in thin layers, vitreous, and quite analogous to what is called glass of antimony.

16. *Seleniuret of Tellurium.*—The two substances unite with the production of heat. The compound is very fusible. When heated, it boils and sublimes in the form of a metallic mass, of a deep brown colour. It is easily oxidized, and leaves on the glass transparent colourless drops, which are not volatile, and appear to be seleniate of tellurium. The sublimed mass melts long before it is heated to redness. It is very liquid, without any tenacity, and cannot be drawn into threads like selenium alone.





*Seleniuret of Ammonia.*—Ammonia does not dissolve selenium either in the state of gas, or in the liquid ; but if we distil muriate of ammonia with seleniuret of lime, we obtain in the receiver a red liquid, having an extremely hepatic taste, which may be mixed with a certain quantity of water without undergoing decomposition, and which, when left in the open air, lets go its ammonia, and leaves the selenium in the form of a grey metallic pellicle. If it be diluted with a great quantity of water, it is decomposed, and has a yellow opalescent colour when viewed by transmitted light ; but a cinnabar-red colour by reflected light. The selenium remains long suspended in it. During the preparation of that compound, a portion is decomposed ; the ammonia is disengaged in the form of gas, and selenium sublimes. Hence it follows that this combination is very weak, and is partly decomposed during its preparation. The matter remaining in the retort after the distillation of seleniuret of ammonia is a mixture of muriate, seleniate, and hydroseleniuret of lime : the last two were formed by the decomposition of a portion of the water of crystallization of the muriate of ammonia. If we dissolve them in weak muriatic acid, the liquid acquires the odour of seleniuretted hydrogen, becomes muddy and red when exposed to the air, and deposits selenium.

*Seleniuret of Lime.*—If we mix selenium in powder with lime, and then expose the mixture to a strong but not a red heat, the two substances combine and form a cohesive black mass, without taste and smell, and which does not dissolve in water. It yields a reddish-brown powder, and on adding an acid, it is decomposed, leaving the selenium in the state of very bulky red flocks. The swelled state of the selenium shows that the mass was a true compound, and not merely lime mechanically mixed with melted selenium.

If we expose this black seleniuret to a red heat, it lets go a portion of its selenium, and its colour becomes lighter. It yields in that state a flesh-coloured powder, but it is equally insoluble and tasteless, as the black seleniuret. We may obtain the same compound by mixing a solution of muriate of lime with that of seleniuret of potash. A seleniuret of lime is formed which precipitates in the form of a red powder.

I obtained the seleniuret of lime crystallized by leaving a solution of hydroseleniuret of lime in an imperfectly stopped vessel, in which it could be slowly oxidized. The liquid by little and little lost its colour, and deposited seleniuret of lime at its surface. At the same time, brown and opaque needles were attached to the sides of the vessel, united together three and three, forming angles of  $120^\circ$ . In some of the groups, there were four or five little crystals. Under the microscope they appeared to be four-sided prisms, with truncated summits. The liquid still contained an excess of lime.



tains twice as much oxygen as the bases, and its capacity of saturation (that is to say, the quantity of oxygen in a base which saturates 100 parts of the acid) is 14.37.

Selenic acid gives salts with two different proportions of excess of acid. In the first, the base is combined with twice as much acid as in the neutral salts; and these compounds are very soluble in water. There are, however, some bases which I have not been able to combine with an excess of acid; viz. oxides of lead, silver, and copper, and protoxide of mercury. In this class, which I shall call *biseleniates*, the acid contains four times as much oxygen as the base. The alkaline biseleniates act as acids, and we cannot produce any intermediate compounds between those which are alkaline and those which are acid, if we except mixtures of the two in such proportions that the two opposite properties are destroyed; but evaporation causes the salt with excess of acid to crystallize, while the other remains in the liquid with its apparent excess of alkali. In the salts with the greatest degree of excess of acid, which I shall call *quadriseleniates*, the base appears to be combined with four times as much acid as in the neutral salts; but I must mention that this conclusion is not founded on researches made on purpose, but is a deduction partly from direct experiments, and partly from analogy. I have produced only quadriseleniates with an alkaline base.

On the other hand, selenic acid seldom gives salts with excess of base, and merely with those distinguished by their tendency to form such salts. For example, oxide of lead does not form a subseleniate when we digest its neutral seleniate with caustic potash; but if we distil the neutral seleniate, it lets go a portion of its acid, and leaves a subsalt from which the acid cannot be expelled by heat. However, with the oxide of copper, we obtain a subsalt even in the humid way. I have not examined the ratio between the acid and base in these subsalts.

The seleniates are usually decomposed by a degree of heat far from considerable. This decomposition is occasioned by foreign combustible bodies which almost always mix with them, especially if they be passed through the filter. The combustible body decomposes a portion of the acid; but the selenium remains in combination with the base. An alkaline seleniate thus treated dissolves in water with a red colour; and an earthy seleniate leaves the selenium in red flocks, when we dissolve it in an acid. If this phenomenon does not appear before heating the seleniate, we mix it with a little nitrate, the acid of which is decomposed in preference.

If we mix an alkaline or alkaline earthy seleniate with charcoal in powder, and then heat it to redness, the selenic acid is decomposed without detonation. Carbonic acid and oxide of carbon are disengaged; a small quantity of selenium sublimes; but the greatest part remains in combination with the base. If



Seleniate of soda was employed by me to determine the degree of saturation of the acid in the alkaline seleniates, and to compare it with what takes place in the earthy and metalline seleniates. The analysis of these salts, however, is not so simple as it appears at first sight.

I attempted to precipitate the solution of the seleniate rendered acid by the muriatic acid necessary to saturate the soda by passing through it a current of sulphuretted hydrogen gas as long as any precipitate fell. But the precipitation was not complete; for on evaporating the liquid, sulphuret of selenium still continued to fall. The muriate of soda, when heated to redness, gave out a strong odour of selenium, and the crucible was attacked. The best mode of performing the analysis is to heat the neutral seleniate of soda with twice its weight of sal-ammoniac. By this means the selenium is driven off, and nothing remains but muriate of soda.

One hundred parts of seleniate of soda that had been strongly heated and then reduced to powder produced, by such an operation, 66½ parts of muriate of soda, equivalent to 35.5 parts of soda. Therefore 100 parts of selenic acid had been combined with 55 parts of soda, containing 14.11 of oxygen; or very nearly half the oxygen contained in the acid.

One hundred parts of the biseleniate, which had been fused under a gentle heat, to drive off the water of combination (but it was impossible to prevent traces of the acid from appearing in the last portions of the water), produced by the same analytical method, 41½ parts of muriate of soda, equivalent to 22.17 parts of soda: 100 parts of the acid, therefore, were combined with 28.48 parts of soda, containing 7.5 parts of oxygen; which exceeds a little half the quantity found by experiment in the neutral salt. I ought to mention that it is very difficult to obtain these salts at their true point of saturation, when we want them dry, since, in the biseleniate, we cannot separate all the water without, at the same time, driving off a small portion of acid. On the other hand, when we endeavour to obtain the neutral seleniate by heating a seleniate containing a small excess of acid, this excess is not driven off, except by a long exposure to the action of the fire.

(To be continued.)

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## ARTICLE V.

*Memoir on a new Acid produced during the Calcination of Mucic Acid.\** By M. Houton Labillardiere.

WHILE treating sugar of milk with nitric acid, Scheele discovered a peculiar acid, to which he gave the name of *sacclactic*

\* Translated from the *Annales de Chimie et de Physique*, ix. 365.





*acid*, because he considered it peculiar to sugar of milk. The same acid was met with afterwards while treating gums with nitric acid, and received the name of *mucons acid*. Finally, the name of *mucic acid* was given to it to render it conformable to the names of the other acids.

Scheele, while examining the properties of this acid, subjected it to the action of heat in a retort, and observed that during its decomposition there sublimed into the upper part of the retort a brown salt, which had a smell similar to that of a mixture of benzoin and amber, which was soluble in water and alcohol, and burned with flame upon red-hot coals; and that the liquid which passed at the same time during the distillation contained the same matter in solution.

Trommsdorf\* resumed the experiments of Scheele on this subject, with a view to examine with care the nature of this crystalline substance, and observed that during the calcination of mucic acid, there were formed succinic, pyrotartaric, acetic acids, an empyreumatic oil, water, and carbonic acid, and carburetted hydrogen gases.

Having read the result of the experiments by which Trommsdorf undertook to prove the identity of this crystalline matter with succinic acid, I observed that in the comparison which he drew between the two bodies, he ascribed properties to succinic acid very different from those which it really possesses; such as the solubility of succinate of copper and of lead: and that the experiments from which he infers the presence of pyrotartaric acid are not more conclusive. These facts induced me to repeat the experiments of these two chemists, and I satisfied myself that during the calcination of mucic acid neither succinic nor pyrotartaric acid was formed; but a peculiar acid to which I give the name of *pyromucic acid*, from the analogy of its formation with that of pyrotartaric acid.

Mucic acid calcined in a retort gives for products a very acid brown liquid, accompanied with some crystals, a small part of which remain attached to the upper part of the retort: it gives also carbonic acid and carburetted hydrogen gases, while a light charcoal remains in the retort. The acid formed in this case is almost completely dissolved in the liquid, and mixed with a little acetic acid and empyreumatic oil, which prevents it from crystallizing. To separate these two matters, the crystals are added to the liquid, and it is mixed with twice or thrice its volume of water, which precipitates a part of the empyreumatic oil, and after having filtered the liquid, it is evaporated till it deposits crystals. The greatest part of the acetic acid separates during the evaporation, and the new acid then crystallizes with facility. After separating the crystals, the mother-water is evaporated again to extract the rest of the acid. In this state, it is in small yellow crystals, which are very difficult to purify by repeated

\* Ann. de Chim. lxxi. 79.

crystallizations, unless we distil them in a small retort furnished with a receiver at a temperature of about  $266^{\circ}$ . They begin to melt, and then distil like chloride of antimony, leaving in the retort merely a small black residue. The distilled acid retains a slightly yellow colour; but a single crystallization is sufficient to render it perfectly pure: 150 grammes of mucic acid give about 60 grammes of the liquid, from which we obtain 8 or 10 grammes of pure acid.

This acid, which I shall distinguish hereafter by the name of *pyromucic*, is white, destitute of smell, of a strong acid taste, melts at the temperature of  $266^{\circ}$ , and is volatilized a little above that temperature, condensing into a liquid, which is converted on cooling into a crystalline mass, the surface of which is covered with fine needles, and it leaves only traces of a residue. When placed on red-hot coals, it melts, and is volatilized in white, penetrating vapours. When exposed to the air, it does not attract moisture. It strongly reddens litmus, is much more soluble in boiling than in cold water. Hot water saturated with it deposits it on cooling in small elongated plates, which cross each other in all directions. Water, at the temperature of  $59^{\circ}$ , dissolves  $\frac{1}{46}$ th of its weight of it. Alcohol dissolves it more abundantly than water.

This acid, subjected to analysis by means of peroxide of copper, gives the following constituents:

	By weight.	By volume.
Carbon .....	52.118	371
Oxygen .....	45.806	120
Hydrogen .....	2.111	84

If we reduce the volumes to more simple numbers, we obtain

Carbon .....	9
Oxygen .....	3
Hydrogen .....	2*

This acid unites readily with the different metallic oxides, and is neutralized by them. Most of its salts are soluble, and capable of crystallizing.

Pyromucate of potash is very soluble in water and alcohol, and crystallizes with difficulty. When evaporated to a pellicle, it assumes the form of a granular mass, which deliquesces in the air.

Pyromucate of soda likewise crystallizes with difficulty,

* This is nearly, 9 atoms carbon.....	= 6.75
6 atoms oxygen .....	= 6.00
2 atoms hydrogen .....	= 0.25
	<hr/>
	13.00

so that if the analysis in the text be correct, the weight of an atom of this acid is 13.—T.



Solutions of copper, protoxide of iron, acetate of lead, zinc, cobalt,	Solutions of silver,  manganese, alumina, magnesia,
--	---

are not altered by these salts ; but they precipitate

Peroxide of iron, yellow ;  
Protonitrate of mercury ;  
Subacetate of lead ;  
Nitrate of tin, white.

The acid alone precipitates only subacetate of lead.

The properties pointed out are sufficient to distinguish this acid from the benzoic and succinic as Scheele may have supposed it to be ; and from the succinic acid alone, which Trommsdorf took it for. As for the pyrotartaric acid announced by this last chemist as one of the products of the calcination of mucic acid, reagents have not been able to ascertain its existence ; while a very small quantity of a soluble pyrotartrate mixed with a great quantity of a pyromucate is demonstrated by the precipitate which it forms with the sulphate of copper.

## ARTICLE VI.

*New Details respecting Cadmium.\** By M. Stromeyer.

M. STROMEYER has communicated to the Royal Society of Gottingen, at the meeting of Sept. 10, 1818, the first part of his researches on the new metal which he discovered in zinc and its oxides, and to which he has given the name of *cadmium*. Assisted by two of his pupils, M. Mahner, of Brunswick, and M. Siemens, of Hamburgh, he has not only verified his first results, but has been able to give a greater extent to his researches, and to reduce them to a great degree of precision. He states that he has explained more fully the circumstances which led to the discovery of cadmium ; and in that way has shown the part which M. Hermann, of Schoenbeck, and Dr. Roloff, of Magdeburg, had in it. He gives likewise the names of the different species of zinc, of its oxides, or of its ores, which contain cadmium. Among these last, M. Stromeyer has found it only in a very small proportion in some blendes, with the exception of some varieties of radiated blende of Przibram, in Hungary, which contains two or three per cent. of it. He like-

\* From *Annalen der Physik*, lx. 193.

270 *M. Stromeyer's new Details respecting Cadmium.* [Oct. wise gives the process for procuring cadmium in a state of purity.

According to this process, we begin by dissolving in sulphuric acid the substances which contain cadmium, and through the solution, which must contain a sufficient excess of acid, a current of sulphuretted hydrogen gas must be passed. The precipitate formed is collected and well washed. It is then dissolved in concentrated muriatic acid, and the excess of acid driven off by evaporation. The residue is dissolved in water, and precipitated by carbonate of ammonia, of which an excess is added to redissolve the zinc and the copper that may have been precipitated by the sulphuretted hydrogen gas. The carbonate of cadmium, being well washed, is heated to drive off the carbonic acid, and the remaining oxide is reduced by mixing it with lamp-black, and exposing it to a moderate red heat in a glass, or earthen retort.

The colour of cadmium is a fine white, with a slight shade of bluish-grey, and approaching much to that of tin. Like this last metal, it has a strong lustre, and takes a good polish. Its texture is perfectly compact, and its fracture hackly. It crystallizes easily in octahedrons, and presents at its surface on cooling the appearance of leaves of fern. It is soft, very flexible, and yields readily to the file, or the knife. It stains pretty strongly; however, it is harder than tin, and surpasses it in tenacity. It is likewise very ductile, and may be reduced to fine wires, or thin plates; yet, when long hammered, it scales off in different places. Its specific gravity, without being hammered, is 8.6040, at the temperature of 62°; when hammered, it is 8.6944. It melts before being heated to redness, and is volatilized at a heat not much greater than what is necessary to volatilize mercury. Its vapour has no peculiar odour. It condenses in drops as readily as mercury, which, on congealing, present distinct traces of crystallization.

Cadmium is as little altered by exposure to the air as tin. When heated in the open air, it burns as readily as this last metal, and is converted into a brownish-yellow oxide, which appears usually under the form of a smoke of the same colour; but which is very fixed. Nitric acid dissolves it easily cold; diluted sulphuric acid, muriatic acid, and even acetic acid, attack it with disengagement of hydrogen gas; but their action is very feeble, especially that of acetic acid, even when it is assisted by heat. The solutions are colourless, and are not precipitated by water.

Cadmium forms only a single oxide; 100 parts of the metal combine with 14.352 of oxygen. According to this, the equivalent number for cadmium is 69.677, and that of the oxide  $69.677 + 10 = 79.677$ . The colour of the oxide varies according to the circumstances in which it is formed. It is brownish-yellow, light-brown, dark-brown, and even blackish. It is quite fixed



and infusible in the strongest white heat, and does not lose its oxygen. When mixed with charcoal, it is reduced with great rapidity before a red heat. It dissolves easily in borax without colouring it, and gives a transparent glass bead. It is insoluble in water; but in some circumstances forms a colourless hydrate, which speedily attracts carbonic acid from the air, and which easily gives out its water when exposed to heat.

The fixed alkalies do not dissolve the oxide of cadmium in a sensible degree, but they promote its combination with water. Ammonia, on the contrary, dissolves it easily. It becomes white in the first place, and is changed into a hydrate. On evaporating the ammonia, the oxide precipitates in a very gelatinous hydrate.

With the acids, the oxide of cadmium exhibits the properties of a saturating base. It forms salts, which are almost all colourless, have a sharp metallic taste, are mostly very soluble in water, and crystallizable, and possess the following characters:

1. The fixed alkalies precipitate the oxide in the state of a white hydrate. When added in excess, they do not redissolve the precipitate, as is the case with the oxide of zinc.

2. Ammonia likewise precipitates the oxide white, and doubtless in the state of hydrate; but an excess of the alkali immediately redissolves the precipitate.

3. The alkaline carbonates produce a white precipitate, which is an anhydrous carbonate: zinc in the same circumstances gives a hydrous carbonate. The precipitate formed by the carbonate of ammonia is not soluble in an excess of this solution. Zinc exhibits quite different properties.

4. Phosphate of soda gives a white pulverulent precipitate. The precipitate formed by the same salt in solutions of zinc is in fine crystalline plates.

5. Sulphuretted hydrogen and the hydrosulphurets precipitate cadmium yellow or orange. This precipitate resembles orpiment a little in colour, with which it might be confounded without sufficient attention. But it may be distinguished by being more pulverulent, and precipitating more rapidly. It differs particularly in its easy solubility in muriatic acid, and in its fixity.

6. Triple prussiate of potash precipitates solutions of cadmium white.

7. Nutgalls do not occasion any change.

8. Zinc precipitates cadmium in the metallic state in the form of dendritical leaves, which attach themselves to the zinc.

The following are the salts which Stromeyer has particularly examined.

Carbonate of cadmium is pulverulent, and insoluble in water. It readily loses its acid when heated. It is composed of

Acid. ....	100.00
Oxide .....	292.88



The sulphate crystallizes in large rectangular prisms, transparent, similar to sulphate of zinc, and very soluble in water. It effloresces strongly in the air, and loses easily its water of crystallization at a gentle heat. It is not decomposed without difficulty by heat, and may be exposed to a feeble red heat without undergoing any change. At a more elevated temperature, it gives out its acid, and is converted into a subsulphate, which crystallizes in plates, and which dissolves with difficulty in water. The neutral sulphate is a compound of

Acid. ....	100·00
Oxide .....	161·12

100 parts of the salt take 34·26 of water of crystallization.

Nitrate of cadmium crystallizes in prisms, or needles, usually grouped in rays. It is deliquescent. Its constituents are:

Acid. ....	100·00
Oxide .....	117·58

100 parts of the dry salt take 28·31 of water of crystallization.

The chloride of cadmium crystallizes in small rectangular prisms, perfectly transparent, which effloresce easily when heated, and which are very soluble. It melts before being heated to redness, after losing its water of crystallization, and on cooling assumes the form of a foliated mass, which is transparent, and has a lustre slightly metallic and pearly; but which speedily loses its transparency in the air, and falls into a white powder. At a higher temperature the chloride of cadmium sublimates in small micaceous plates, which have the same lustre and transparency as the melted chloride, and which undergo the same alteration in the air: 100 parts of fused chloride are composed of

Cadmium .....	61·39
Chlorine. ....	38·61
	<hr/>
	100·00

Phosphate of cadmium is pulverulent, insoluble in water, and melts, when heated to redness, into a transparent vitreous body. It is composed of

Acid. ....	100·00
Oxide .....	225·49

Borate of cadmium obtained by precipitating a solution of neutral sulphate of cadmium by borax is scarcely soluble in water. When dry, it is composed of

Acid .....	27·88
Oxide. ....	72·12
	<hr/>
	100·00

Acetate of cadmium crystallizes in small prisms, usually disposed in stars, which are not altered by exposure to the air, and are very soluble in water.

Tartrate of cadmium crystallizes in small needles, soft, like wool, and scarcely soluble in water. The oxalate is pulverulent and insoluble.

The citrate forms a crystalline powder, very little soluble.

Cadmium combines with sulphur, as with oxygen, in only one proportion. One hundred parts of cadmium take 28.172 of sulphur. This sulphuret has a yellow colour, with a shade of orange. When heated, it becomes first brown, and then crimson, but it loses these colours on cooling. It is very fixed in the fire. It begins to melt when it is heated to a white-red. It then crystallizes on cooling in transparent, micaceous plates, of the finest lemon-yellow colour. It dissolves even cold in concentrated muriatic acid, with the disengagement of sulphuretted hydrogen gas; but it is attacked with difficulty even with the assistance of heat, when the acid is diluted.

Sulphuret of cadmium is formed difficultly by fusing together the metal and sulphur. It is obtained with much greater ease by heating together a mixture of sulphur and oxide of cadmium; or by precipitating a salt of cadmium by sulphuretted hydrogen.

This sulphuret, from its beauty and the fixity of its colour, as well as from the property which it possesses of uniting well with other colours, and especially with blue, promises to be useful in painting. Some trials made with this view gave the most favourable results.

Phosphuret of cadmium obtained by combining the metal with phosphorus has a grey colour, and a lustre feebly metallic. It is very brittle, and uncommonly refractory. When put upon a red-hot coal, it burns with a beautiful phosphoric flame, and is converted into a phosphate. Muriatic acid decomposes it, disengaging phosphuretted hydrogen gas.

Iodine unites with cadmium both in the dry and moist way. We obtain large and beautiful hexahedral tables. These crystals are colourless, transparent, not altered by exposure to the air. Their lustre is metallic, approaching to pearly. It melts with extreme facility, and assumes on cooling the primitive form. Exposed to a higher temperature, it is decomposed, and allows iodine to escape. Water and alcohol dissolve it with facility. It is composed of

Cadmium . . . . .	100.00
Iodine . . . . .	227.43

Cadmium unites easily with most of the metals, when heated with them without contact of air, in order to avoid oxidation. Most of its alloys are brittle, and colourless; but hitherto only a few have been examined with precision.

The alloy of copper and cadmium is white, with a slight tinge

## ARTICLE VIII.

*On Lasionite and Wavellite.* By Dr. Joh. Nep. Fuchs, Professor of Chemistry and Mineralogy at Landshut.

IN the 18th volume of Schweigger's Journal, p. 288, I inserted a short notice of a mineral, to which, from its hair-form crystallization, I have given the name of *lasionite*. It is found in the ironstone mine of St. Jacob, in the Upper Palatinate, where it occurs very sparingly scattered in the brown ironstone. This mineral has been long known and considered as a feather zeolite, to which, in its external characters, it has a very strong resemblance. I found its constituents to be *alumina*, *phosphoric acid*, and *water*. This authorized me to make a peculiar genus of it, as no mineral composed of the same constituents had been hitherto known. It occurred to me, however, that *wavellite*, which I had not yet seen, might be nearly of the same nature with it; but I could not listen to this suspicion, as three of our most celebrated chemists, Klaproth, Davy, and Gregor, who had analyzed this mineral, had found nothing in it but alumina and water. Since that time, however, I have satisfied myself by experiment that wavellite not only contains phosphoric acid, but that in its chemical composition, it agrees exactly with lasionite. What follows will show this.

*Examination of Lasionite.*

Lasionite is infusible before the blow-pipe; but it gives the flame a bluish-green colour, and thus betrays the presence of phosphoric acid. With carbonate of soda, it froths, and melts into a doughy mass. When heated to redness, it loses, according to a careful and very accurate experiment, 28 per cent. of its weight. It dissolves completely in muriatic and nitric acid, as well as in caustic potash or soda; but the solution takes place much more easily and rapidly in the alkalies than in the acids, which must be employed in considerably greater quantities. When acetate of lead is dropped into the nitric acid solution, only a very small precipitate of phosphate of lead falls, when the excess of acid has been previously saturated with ammonia. Sal-ammoniac throws it down unaltered from the alkaline solutions, and when ammonia is digested on the precipitate, it takes up but a very small proportion of the phosphoric acid. This shows us the difficulty of completely separating the phosphoric acid from the alumina. I was not completely successful in accomplishing this separation in my former experiments. By repeating my experiments, which I have been enabled to do by the goodness of Prof. Graf, who sent me some new specimens of the mineral from a fresh opening, I have obtained results which differ considerably from my former ones. I performed the

analysis in the following way: I dissolved 25 gr. in caustic potash, and added a small additional quantity of potash to the solution, poured into it a solution of muriate of lime prepared with 25 gr. of carbonate of lime, and allowed the mixture to digest for a short time. The whole was then thrown upon the filter, and the alumina separated from the liquid which passed through by means of sal-ammoniac. It weighed, after being dried in a red heat, 9.14 gr. did not tinge the flame of the blow-pipe, and with sulphuric acid and potash it formed alum.

The precipitate obtained by means of the muriate of lime, which consisted of phosphate of lime with excess of base, dissolved with slight effervescence in muriatic acid. From this solution the phosphate of lime was precipitated by means of caustic ammonia, separated on the filter, well washed with hot water, dried, and heated to redness. It weighed 19.2 gr. had a gummy-like appearance, was infusible, dissolved without effervescence in nitric acid, and acetate of lead threw down a copious precipitate from the solution, which, when exposed to the flame of the blow-pipe on charcoal, melted into a shining polyhedral button. After these trials, I held it superfluous to seek any further evidence of the existence of phosphoric acid in it; but it was still uncertain how much of this acid, phosphate of lime contains. To ascertain this, I dissolved 50 gr. of Iceland spar in muriatic acid, mixed the solution (after driving off the excess of acid by evaporation) with some caustic ammonia, and precipitated the phosphate of lime by means of phosphate of ammonia.\* It possessed exactly the qualities of the phosphate of lime obtained in the analysis of the lasionite. After being heated to redness, it weighed 51.52 gr. If we allow calcareous spar to contain 56.4 per cent. of lime, it follows that our phosphate of lime, which possesses the characters of a neutral salt, is composed of

Phosphoric acid .....	45.26
Lime .....	54.74
	<hr/>
	100.00

This very nearly corresponds with Klaproth's analysis of apatite. Hence it follows that 19.2 gr. contain 8.68 gr. of phosphoric acid, and the result of the analysis of 25 gr. of lasionite is:

Alumina .....	9.14
Phosphoric acid . . . . .	8.68
Water .....	7.00
	<hr/>
	24.82

\* When the two neutral solutions are mixed together, we obtain a slimy, pearly powder, which acts as a weak acid, melts easily before the blow-pipe, and gives the flame a bluish-green colour. This salt, which contains much more phosphoric acid than that obtained in the way described in the text, is a biphosphate of lime. We have here an example of Richter's well-known law.



Therefore 100 parts contain :

Alumina . . . . .	36.56
Phosphoric acid. . . . .	34.72
Water. . . . .	28.00
	<hr/>
	99.28.

*Examination of Wavellite.\**

Wavellite, to which the names of devonite and hydrargillite have been also given, in its physical properties does not differ from lasionite. Before the blow-pipe it exhibits the same characters, loses the same weight in the fire, and is acted on in the same way by the acids and alkalies.

The first analysis which I made of it was performed in the same way as the preceding, but the result was very different, as I obtained 54.32 per cent. of alumina, and 25.68 per cent. of phosphoric acid. When to these we add 28 per cent. of water, there is an excess of 8 per cent.; but it was not difficult to explain whence this difference and this excess proceeded. I had employed a greater quantity of potash in this analysis than in the preceding, and I boiled it for an hour over the precipitated phosphate of lime. This occasioned the solution of some phosphate of lime in the potash liquid which contained the alumina; and this portion was mixed with the alumina when that earth was thrown down by sal-ammoniac. When now the eight parts of excess are considered as lime, and the phosphoric acid combined with it, and dissolved in the potash ley along with it, is considered as forming not neutral, but, as is probable, biphosphate of lime, which, according to my experiments, contains 53.4 per cent of phosphoric acid, and of course must be composed of 8 lime + 9.16 acid = 17.16 parts. This, being subtracted from the alumina, and 9.16 parts being added to the phosphoric acid, we obtain the constituents of wavellite as follows :

Alumina. . . . .	37.16
Phosphoric acid . . . . .	34.84
Water. . . . .	28.00
	<hr/>
	100.00

As doubts might be entertained respecting the accuracy of these numbers, I considered it as necessary to repeat the analysis; and to avoid the uncertainty produced by the solution of the phosphate of lime in the potash ley, I proceeded in quite a different way.† I employed for the decomposition a solution

\* The specimen of wavellite which I employed in this analysis was from Barnstable. I was indebted for it to Major Petersen.

† Theodore de Saussure, as far as I know, was the first person who observed that phosphate of lime is soluble in potash.—(See Gehlen's *Journal für die Chemie und Physik*, li. 668—702.) Klaproth likewise, in his experiments on the phosphoresc-

of silica in potash, which I had found to answer very well in former experiments. Twenty-five grains of wavellite were dissolved in potash ley, and this solution was mixed with liquid silicate of potash, which contained an equal quantity of silica. There was immediately formed a thick slimy matter, from which by dilution with water, and boiling, a copious precipitate fell, to which I shall give the name of A. The liquid filtered from this precipitate was mixed with sal-ammoniac, without becoming muddy. It was then evaporated to dryness. The residual saline mass dissolved completely in water, and was neutral. It was mixed in the first place with some ammonia, and then with muriate of lime, which occasioned a copious precipitate. This precipitate was separated immediately by the filter, washed with hot water, dried, and heated to redness. It weighed 19·4 gr. and exhibited the properties of neutral phosphate of lime. It dissolved readily and completely in nitric acid; and during the solution some small air-bubbles were extricated. It was then thrown down from the acid in the state of phosphate of lead.

The precipitate A, which contained the alumina united to silica,\* was carefully collected, and treated with muriatic acid. It dissolved rapidly and completely, and the solution assumed the form of a stiff jelly. From this the silica was separated in the usual way. The alumina was then thrown down from the solution by ammonia. It weighed 9·3 gr.

As 19·4 gr. of phosphate of lime contain 8·78 gr. of phosphoric acid, we have the constituents of wavellite as follows:

Alumina .....	9·30
Phosphoric acid .....	8·78
Water .....	7·00
	<hr/>
	25·08

Consequently 100 parts contain,

Alumina. ....	37·20
Phosphoric acid. ....	35·12
Water. ....	28·00
	<hr/>
	100·32

The small excess may proceed from a little carbonate of lime

ing earth from Marmarosch, obtained a precipitate of phosphate of lime, which dissolved in potash. (See *Beitrag*, iv. 366.) It is to be presumed that this precipitate contained phosphate of alumina.

\* I must here remark, that when a solution of silica and of alumina in an alkali are mixed together, the two earths do not precipitate alone, as has hitherto been thought, but united with a considerable quantity of the alkali. Hence the reason why the precipitate dissolves completely in acids and forms a jelly with them, as is the case with natrolite and fatstone. If soda be employed in this experiment, we obtain an *artificial natrolite*. I have made more observations on this important property in my *Memoir on the Origin of Porcelain Earth*, which will appear in the *Transactions of the Academy of Sciences in Munich*.





which was mixed with the phosphate of lime, and made the quantity of phosphoric acid be stated a little above the truth. In other respects, this result agrees very well with the preceding, and with that which the lasionite gave. We are entitled from it to conclude, that wavellite is not a hydrate, but a phosphate of alumina, and constitutes only one genus along with lasionite, which I propose to distinguish by the euphonious word *lasionite*. I examined a portion of this mineral for fluoric acid, but could discover no trace of it.

In my former notice, I stated a conjecture that phosphoric acid may constitute an ingredient of many minerals, and when it is in combination with alumina, it may be easily overlooked by chemists; because phosphate of alumina and pure alumina have the same solvents and the same precipitants, and alum may be formed from the one as well as the other. I have now more reason to repeat this conjecture, seeing that the presence of phosphoric acid has been overlooked in wavellite, though it exists in that mineral in such considerable quantity. The chief reason of this oversight was, that the alumina which it contained formed alum with sulphuric acid and potash. The formation of alum then is no sure indication of the purity of alumina, and shows only that alumina is present. If phosphoric acid be in combination with it, this acid may be either separated by the sulphuric, or it may enter as an ingredient along with it into the alum; a point which can only be determined by an experimental investigation. Its presence in the alumina betrays itself, as has been already remarked above, when the mineral is subjected to the action of the blow-pipe by the bluish-green colour which it gives to the flame. This happens even when the proportion of phosphoric acid is very small.\* The examination of alumina, therefore, for phosphoric acid is easy, and no analyst will overlook it hereafter. But this is not sufficient. According to the common method of separating silica, phosphate of alumina may easily be confounded with it. Hence it will be necessary to apply the same test to the silica. And as the alumina may conceal other substances as well as phosphoric acid, it will be necessary, by analytical trials, to correct our notions of the characters of this earth. Above all, the analysis of sapphire and corundum ought to be repeated, as it is not improbable that these minerals, together with alumina, may contain some other substance; perhaps a metallic oxide. Probably one or other of the new earths might, by a more accurate analysis, be divided into alumina and some other substance.

\* The compounds of phosphoric acid which, like apatite, do not colour the flame of the blow-pipe, acquire that property if they be plunged into sulphuric acid. This, therefore, is a very good method of knowing the presence of phosphoric acid. It may be employed likewise for the compounds of boracic acid, which give the flame a much finer, livelier, and purer green colour than phosphoric acid.

I consider it as nearly superfluous to say any thing about the place which lasionite should occupy in the mineral system; as this presents itself almost spontaneously; and every mineralogist will be able, without difficulty, to give it the place to which it belongs in the system that he has adopted. Lasionite must be ranked among the salts, and when these are divided into genera, according to the acids which they contain, which, in my opinion, is the best and most convenient method, it will come as a phosphate, and be placed immediately after apatite.

Lasionite may be formed artificially by various methods; but it can be exhibited only in a pulverulent form, like the earthy wavellite when it occurs in that state.\* We obtain it when we dissolve fresh precipitated alumina in phosphoric acid, and precipitate by means of ammonia; probably likewise when we decompose an aluminous salt, as alum, by means of phosphoric acid, or a phosphate soluble in water, in the usual way, and then pour ammonia into the liquid. When we mix together a solution of alum and phosphate of ammonia, a slight muddiness occurs at first, which speedily disappears. From this mixture, phosphate of alumina may be precipitated by acetate of ammonia, even when an excess of acetic acid is present. This mode of proceeding is probably the best, because even when an excess of alum is present, it will not occasion the precipitation of any uncombined alumina. The analysis of artificial lasionite gave a result very nearly the same with that of the natural mineral; only the loss of weight in the fire was considerably greater, although it had been well dried before it was exposed to a red heat.

## ARTICLE IX.

*Observations on the Relation of the Law of Definite Proportions in Chemical Combination, to the Constitution of the Acids, Alkalies, and Earths.* By John Murray, M.D. &c. &c. Read before the Royal Society of Edinburgh, March 2, and May 18, 1818. (Not yet published.)

THE law that every body enters into chemical combination in a certain equivalent weight to others, and that when it combines in different proportions with another, these proportions have a simple arithmetical ratio, is perhaps the most important that has hitherto been discovered in the science of chemistry. It is now so far established, notwithstanding some difficulties which attend it, that when a view of the constitution of an extensive series of

\* The so called earthy talc, from Freiberg, which H. John analyzed (Schweigger's Journal, v. 222), may, perhaps, belong to it; but I can determine nothing on that point, as I am not possessed of the mineral.

chemical compounds is brought forward, different from what had hitherto been proposed, it is incumbent to show that it is consistent with the operation of this law; and if just, this may display relations not before observed, and may obviate objections which have arisen from a different view. It is from these considerations that I submit the following observations on the application of the law of definite proportions to the theory which I have proposed of the chemical constitution of the acids, alkalies, and their compounds. It necessarily leads to considerable modifications of these applications; and the conclusions which these afford, if I am not deceived, afford proofs of the truth of the opinion I have advanced, and lay open some new views. The subject is at the same time so extensive as to have relations to nearly all the details of chemistry.

In the preceding paper I remarked, that the relations in the proportions of oxygen and hydrogen forming the supposed portion of combined water in the acids, will probably be those of one or both of these elements directly to the radical. It remained to be determined how far this is just.

Sulphur affords the best example for illustration, as its combinations with oxygen and hydrogen are capable of being accurately determined.

Sulphur and oxygen are held to combine in two definite proportions, forming sulphurous and sulphuric acids. In the first, 100 parts of sulphur are combined with 100 of oxygen; in the second, 100 are combined with 150 of oxygen, forming what is called the real acid, with which are further combined 56.7 of combined water, the entire compound, constituting the acid in the highest state of concentration (1.85 of specific gravity), in which it can be procured in an insulated form.

This constitution of these compounds appears at first view in opposition to the law of definite proportions in chemical combinations; for, according to that law, the higher proportion of an element in combination with another is a simple multiple of the lower proportion in which it combines with the same body; and hence, since in the first combination of sulphur with oxygen, 100 of the former are combined with 100 of the latter; in the second, 100 ought to be combined with 200, while the combination is that of 100 to 150. And in the atomic hypothesis, this involves the absurdity of supposing, that while, in the first compound, the combination, in conformity to the common rule, is that of one atom of sulphur with one atom of oxygen; in the second, it is that of an atom of sulphur with an atom and a half of oxygen. To obviate this, it is supposed that a combination of sulphur with a lower proportion of oxygen exists—an oxide composed of 100 of sulphur with 50 of oxygen. The ratio will then be that of 1, 2, 3, of oxygen in the three compounds to one of sulphur. And in the atomic system, the first will be held to be that of an atom of sulphur with an atom of oxygen; the

second, that of an atom with two atoms ; and the third, that of one with three. To this, however, it may be objected, that no such oxide of sulphur can be obtained ; though, if it were a possible combination, it ought, from the law of attraction, that the first proportion of an element is retained in union with the greatest force, to be the one most permanent and most easily obtained.

Whether this objection be just or not, the difficulty can be solved without any hypothesis, on the view that the elements of the supposed water exist in the composition of the acid ; for the quantity of oxygen in this water is just 50 : of course, the entire quantity is the regular proportion 200. And the composition is 100 of sulphur, 200 of oxygen, and 6·7 of hydrogen.

This result favours the conclusion, that the relation of the oxygen in common sulphuric acid is entirely that of this element to sulphur ; that it is, therefore, in immediate combination with the radical ; and hence, that water does not exist in the constitution of the acid. And even if the existence of an oxide of sulphur, and of what is called real sulphuric acid, were admitted, the combinations would be strictly conformable to the law of proportions, being those of one of sulphur to 1, 2, 3, and 4 of oxygen.

The proportion of hydrogen is also determined by its relation to the sulphur, for its quantity is that in which they combine, 6·7 of hydrogen with 100 of sulphur, constituting the composition of sulphuretted hydrogen.

It thus appears that the proportions of both elements are determined by their relation to the sulphur as the radical of the acid, and are those which the quantity of sulphur would separately require. This, so far as theory can discover, is not a necessary result. The oxygen and hydrogen might each have required the quantity of sulphur with which they combine ; that is, the existing relations might have been those of sulphur to oxygen, and sulphur to hydrogen, in their several proportions. It is otherwise ; there is the relation of sulphur to oxygen, and in addition to this of hydrogen to the same sulphur ; and thus, since the same quantity of sulphur receives the acidifying influence of both elements, we discover the source of the higher degree of acid power. How water should augment acidity, no principle enables us to conjecture. But how the joint operation of two elements acting on the same quantity of radical which each of them separately is capable of rendering acid, should augment the effect is easily perceived. And even from this consideration alone, there can remain little hesitation in admitting the conclusion, that both these elements act directly on the sulphur ; in other words, that the three are in simultaneous combination.

As there is no proof of the existence of oxide of sulphur, and as no such compound as that denominated real sulphuric acid,



composed of 100 of sulphur with 150 of oxygen, can be obtained insulated, it might be supposed that the hypothesis of such combinations ought to be excluded; and that the strict fact only should be admitted, of the two compounds which constitute sulphurous and sulphuric acids.

There is one ground, however, on which it may be inferred that a relation of sulphur to oxygen, in the proportion of 100 to 150, exists. When sulphuric acid is acted on by a base neutralizing it, its hydrogen combines with a portion of its oxygen forming water. The quantity of oxygen thus abstracted is 50, and, of course, the above proportion remains; and this being admitted, the existence of oxide of sulphur, it may be supposed, must also be assumed to bring the results under the law of definite proportions; and the combinations of oxygen to sulphur will still be in the ratio of 1, 2, 3, 4.

This conclusion, however, does not follow; for in cases where this apparent result happens, the oxygen which is abstracted forming water is replaced by the oxygen of the base, and makes up the proportion of 200 to 100 of sulphur; and the new compound is a ternary combination of these elements in these proportions with the metallic radical of the base. A single example will illustrate this: 30.7 of common sulphuric acid require for saturation 69.6 of oxide of lead; the former is composed of 10 of sulphur with 20 of oxygen, and 0.7 of hydrogen; the latter of 64.6 of lead with 5 of oxygen. The hydrogen in their mutual action abstracts 5 of oxygen forming water, and there remain 20 of oxygen, 10 of sulphur, and 64.6 of lead in combination. The same result is established in all other cases, and they afford no evidence, therefore, of the existence of any such compound as that of real sulphuric acid.

But there is another case which does not admit of the same explanation, and in which the relation of 1 of sulphur to  $1\frac{1}{2}$  of oxygen seems to be demonstrated. It is that of the action of sulphurous acid on salifiable bases. Here, as there is no abstraction of oxygen in the formation of water, while there is the addition of the oxygen of the base, the proportion in the combination is that of  $1\frac{1}{2}$  to 1 of sulphur. This will be apparent from the same example of oxide of lead: 20 of sulphurous acid composed of 10 of sulphur and 10 of oxygen combine with 69.6 of oxide of lead, composed of 64.6 of lead and 5 of oxygen: supposing a simultaneous combination to be established, the proportions will be 10 of sulphur, 15 of oxygen, and 64.6 of lead; and supposing the two latter to observe a relation to sulphur, the proportion is that of 100 to 150 of oxygen.

It might be maintained that no change of composition in the two binary compounds, the sulphurous acid and oxide of lead, takes place, but that they merely unite; or at least that while the sulphur and lead display their peculiar relation to each other, each of them retains its relation to oxygen. But this is incon-

sistent with the general view which has been given of the state of a neutral compound, and can scarcely be supposed to exist with regard to one case, when the reverse is maintained with regard to others.

At the same time, the relation of 100 of sulphur to 200 of oxygen is fully established in common sulphuric acid. Whether it is necessary to admit that of 100 to 50, except on the atomic hypothesis, is not apparent, but it is not improbable.

The same view may be applied to the illustration of the acids of which carbon is the radical. I have remarked in the preceding paper, that the vegetable acids are to be regarded, not according to the doctrine of Lavoisier, as composed of a compound radical of carbon and hydrogen acidified by oxygen, but as compounds of a simple base, carbon, acidified by oxygen and hydrogen. On this principle the question occurs, what is their precise composition? The proportions assigned by the analyses hitherto given appear at variance with every principle, and can be brought under no law, nor any apology whatever; nor has this been attempted. Part of this may arise from the difficulties of the analysis, but more of it, perhaps, is to be ascribed to the composition not having been considered under the just point of view; in more recent investigations, particularly in which only accurate experimental results can be expected, to the idea having been entertained that they contain a portion of combined water in their insulated state, which they yield when combined with a base, and that the composition of the acid is to be determined, abstracted from this water, and as it exists in combinations in which it is supposed to be in what is called its real state. The principle which I have applied to their constitution leads to very different results.

In conformity to the law, which it has been shown exists with regard to sulphur, it is probable the oxygen and hydrogen will be in the definite proportions which they separately observe to carbon; and from the different proportions in which they combine with this element, a number of compounds may be thus formed.

Carbon, with the first proportion of oxygen, forms an oxide. Hydrogen is an acidifying power. Its addition, therefore, it is not improbable, may give rise to acidity, and its proportion will be determined either by its first or second proportion to carbon, or by both. Carbon, with its second proportion of oxygen, forms a weak acid. The addition of hydrogen to this will no doubt augment acidity, and its proportion will also be determined by its first or second proportion to carbon, or both. Four specific compounds will thus be established, which will be represented by carbonic oxide with a certain proportion of hydrogen, one that which exists in carburetted hydrogen, the other that in supercarburetted hydrogen; and by carbonic acid with similar proportions of hydrogen. Further, there has appeared reason to



infer the existence of a relation in proportion of sulphur to oxygen intermediate between that of sulphurous and sulphuric acid ; a similar relation may exist in the case of carbon, intermediate between carbonic oxide and carbonic acid ; and with the addition of hydrogen, may give rise to acidity. Lastly, there is some reason also to suppose the existence of a combination of sulphur with oxygen in a lower proportion than that in sulphurous acid. There may be a similar combination with carbon, which may also, with an additional proportion of hydrogen, produce acidity. It remains to inquire how far the composition of any of the vegetable acids can be brought under these laws.

Carbonic acid is the binary compound of carbon and oxygen. With the addition of hydrogen there is every reason to infer, that, as in the case of all the other binary acids containing oxygen, an acid will be formed of increased power. Oxalic acid is the strongest of the vegetable acids ; and the results of its analysis will be found to lead to the conclusion that it is this ternary compound.

Berzelius submitted oxalic acid to experiment by combining it with oxide of lead, drying the oxalate, and decomposing it by heat. His object in following this method was to abstract the combined water of the acid, and to operate upon it in what is considered as its real state. He accordingly found, that the acid loses water in entering into this combination ; and he objects to a preceding analysis by Gay-Lussac, in which the oxalic acid had been operated on in the state of oxalate of lime, as in this combination the water of composition is not abstracted. His objection is valid, on the doctrine which has been universally adopted by chemists, of acids containing water essential to their constitution, which is abstracted when they enter into combination with a base, such as oxide of lead, in which water is not retained. And if oxalic acid in passing into this combination lose water, as is the case, then, on this idea, its constitution ought to be determined from its analysis as it exists in a dry oxalate, exactly as that of sulphuric acid is inferred from its analysis in the state in which it exists in a dry sulphate. The reasoning of Berzelius, therefore, was relatively just ; and on these data his results, though they have been objected to, as they involve difficulties in the atomic hypothesis, are correct. But in conformity to the doctrine I have illustrated, it is evident that the composition of the acid is not thus obtained, and that what exists in a dry oxalate, such as oxalate of lead, is a different combination. The crystallized oxalic acid is what ought to be submitted to analysis if it contained no water of crystallization ; but as it does contain a portion, this is to be removed, without abstracting what has been called water essential to the acid. It exists in this state in oxalate of lime ; and hence the results given by Gay-Lussac (if experimentally correct, and they appear to be singularly so) give its real composition. They are

accordingly strictly conformable to the view I have stated of the composition of this acid. The proportions he assigns are 26.56 of carbon, 70.69 of oxygen, and 2.75 of hydrogen.\* Now carbonic acid is composed of 27.4 of carbon, and 72.6 of oxygen. The proportion of carbon and oxygen, therefore, in oxalic acid is precisely the same; and the sole difference in composition from carbonic acid is in the proportion of hydrogen it contains.

The constitution of oxalic acid may likewise be inferred indirectly from the method of Berzelius; and it will be satisfactory if a coincidence is thus obtained. The composition of the real acid, as it is called, existing in oxalate of lead, is stated by Berzelius at 33.22 of carbon, 66.53 of oxygen, and 0.25 of hydrogen. But to this, to express the true composition of the acid, are to be added the proportions of oxygen and hydrogen expended in the formation of water, in the mutual action of the acid and the oxide of lead. The quantity of hydrogen is inferred from the quantity of oxygen: and there are different principles connected with the doctrine, as has been already illustrated in considering the action of sulphuric acid on a base, whence the proportion of oxygen may be determined. Thus, it must be a multiple of that existing in the composition of what is called the real acid, or in the composition of the known definite compounds of carbon and oxygen, or it is equivalent to the oxygen in the base, this quantity of oxygen being always abstracted in the mutual action in combination with the requisite proportion of hydrogen. Adopting this last principle as the most direct, 100 parts of real oxalic acid, it appears from Berzelius's analysis, combine with 307.5 of oxide of lead: this quantity of oxide contains 22.06 of oxygen, which is, therefore, to be added to the composition of the acid, with the proportion of hydrogen equivalent to this, 2.94. Hence this quantity of acid, 125 parts, is composed of carbon 33.22, oxygen 88.59, hydrogen 3.19: or in 100 parts the acid consists of 26.5 of carbon, 71 of oxygen, and 2.5 of hydrogen, proportions almost the same as those assigned by Gay-Lussac, and affording a coincidence on a difficult subject of experimental investigation that does honour to the accuracy of these chemists.

There can thus remain no doubt that the proportion of carbon to oxygen in oxalic acid is the same as that in carbonic acid. The sole difference between them is in the proportion of hydrogen which the former contains: the one is the binary, the other the corresponding ternary compound, similar to what exists in other acids; and hence also, in conformity to the analogy of these acids, and to the principle which accounts for their acidity, is explained the difference in their acid powers.

The compound existing in a dry oxalate, such as oxalate of lead, ought to contain no hydrogen; for the whole of this ele-

\* *Recherches Physico-Chimiques*, tom. ii. p. 302.

ment, like the hydrogen of sulphuric acid, must, in the action of the base, be combined with oxygen, and abstracted in the state of water. The small portion of hydrogen, therefore, stated by Berzelius, must be considered as derived from error of experiment; and its presence would be admitted more readily from the idea of some portion of hydrogen being essential to the constitution of the acid, as necessary to form what was regarded as its compound radical. In subsequent experiments, accordingly, Berzelius found reason to infer that the proportion was smaller than he had at first assigned. The minute quantity which he does suppose to exist in real oxalic acid (less than one per cent.) he brings forwards as a difficulty in the atomic hypothesis. A fraction of an atom, he remarked, cannot be supposed; and, therefore, the small quantity of hydrogen must be considered as an entire atom. But from the proportions, it must be held to be combined with 27 atoms of carbon, and 18 atoms of oxygen, that is, with 45 other atoms—a combination certainly altogether improbable; and any arrangement that can be conceived scarcely lessens the difficulty. Mr. Dalton endeavoured to obviate this, by supposing that in the analysis of Berzelius the hydrogen is underrated. But the reverse is the case. The solution may now be easily given. In the composition which properly constitutes oxalic acid, the proportion of hydrogen is sufficiently large to present no difficulty. And in what was considered as real oxalic acid existing in the dry oxalates, there is no reason to suppose that hydrogen exists. It is also obvious, that the proportion of oxygen and carbon in a dry oxalate is that constituting carbonic acid; for although in the action of the acid on the base a portion of its oxygen is abstracted with its hydrogen, a corresponding portion of oxygen is added from the base or metallic oxide, and a ternary compound is established.

The proportion of hydrogen indicated in the composition of oxalic acid is not conformable to either of the two proportions of carbon and hydrogen, which constitute the two compounds at present admitted as constituting the only definite compounds of these elements, the carburetted and supercarburetted hydrogen. It is much less even than that in the latter, which contains the lower proportion. Yet there is every reason to conclude, from the law which has been illustrated in reviewing the composition of sulphuric acid, that it must exist in a definite relation to the simple radical of the acid, that is, to the carbon, conformable to the other relations which subsist between them. It follows, therefore, either that there is an error of analysis, in consequence of which the proportion of hydrogen is greatly underrated, or that there are other definite proportions in which carbon and hydrogen combine than those which are at present admitted. The coincidence in the results of the analysis by Gay-Lussac and by Berzelius, in a great measure precludes the former supposition; and indeed an error so great would require

to be assumed as cannot be supposed. The other conclusion, therefore, follows : it is rendered more probable by other considerations, which give force to the opinion that hydrogen and carbon enter into more numerous proportions than have been assigned ; and it is nearly established by the results of this case itself. Supercarburetted hydrogen is composed of 100 of carbon with 17·5 of hydrogen ; carburetted hydrogen of 100 with 35. In oxalic acid, 26·5 of carbon are combined, according to the analysis of Berzelius, with 2·5 of hydrogen, which is in the proportion of 100 to 9·4. Now this deviates little, and not more than what may fairly be referred to inaccuracy in the estimation of the proportions in one or other of the compounds, from the fourth of the highest proportion, that in carburetted hydrogen ;\* and hence, in conformity to the law usually observed, hydrogen probably combines with carbon in proportions following the ratio of 1, 2, 3, 4 ; and taking a mean which further investigation may determine with precision, 100 of carbon may be supposed to combine with 9, 18, 27, and 36, of hydrogen. The proportion in oxalic acid will be conformable to the first of these relations, or half that in supercarburetted hydrogen.

Tartaric acid, which is next in strength to the oxalic, or is even equal or superior to it in acidity, appears to be the same combination with a larger proportion of hydrogen.

Gay-Lussac employed tartrate of lime as the medium to decompose the acid. In this state, while the water of crystallization of the acid is excluded, its composition is not subverted, for there is in the formation of tartrate of lime no abstraction of what is called combined water. The results, therefore, give the real constitution of the acid. The proportions he assigned are carbon 24·05, oxygen 69·3, hydrogen 6·62. Berzelius operated on tartrate of lead. The proportions he assigns are carbon 35·98, oxygen 60·21, hydrogen 3·807. But in the formation of tartrate of lead by the action of the oxide on the acid, a large quantity of water is formed. This being taken into calculation, his results agree perfectly with those of Gay-Lussac.

The proportion of the carbon to the oxygen, it is evident, is not much different from that which constitutes carbonic acid ; and the deviation is not greater than may fairly come within the allowance due to errors, liable to be present in a subject of analysis so difficult.

The proportion of hydrogen is much larger than that in oxalic acid : it must, however, in conformity to the law which has been stated as regulating the proportions in ternary acids, bear a certain relation to the radical of the acid, that is, to the carbon. And it is interesting to discover that this larger quantity is precisely the other definite proportion which it has appeared

\* The composition of either of the carburetted hydrogen gases is not so well determined as to exclude a correction sufficient to establish a perfect coincidence.



from these illustrations must be inferred to exist in the combinations of carbon and hydrogen. The two known proportions, those existing in supercarburetted and carburetted hydrogen, are 100 of carbon to 18 of hydrogen, and 100 to 36; the other two are those of 9 and 27. The first was found in oxalic acid, and the other is discovered in tartaric acid, the proportion in the above analysis of 24.05 to 6.62, being that of 100 to 26.5.

In the remaining vegetable acids, the composition is evidently less perfectly determined, partly from the difficulty of procuring them insulated, and partly from the sources of error which attend the experiment, and which have not been checked or detected by the application of a just principle. It is, therefore, only from repeated experimental investigation, aided by such an application, that precision can be expected to be obtained. Still some of these results afford very high approximations to the views I have illustrated.

The proportions I assign are those founded on the analyses by Berzelius, corrected by the theory I have stated. He combined the acid with oxide of lead, and submitted it to decomposition in this state; the water of composition he supposed to be thus abstracted, and the real acid obtained. But the composition of the acid is in fact subverted, and the water is formed from the combination of its hydrogen with a portion of its oxygen. The quantity of oxygen thus lost is discovered by the quantity of oxide which the acid saturates, being equal, according to the principle already explained, to the quantity of oxygen in the oxide. The hydrogen lost is the quantity equivalent to this; and these quantities of oxygen and hydrogen being added to the proportions assigned by Berzelius, give the real composition. It is further necessary to remark, that as there has appeared reason to infer the existence of four definite proportions of oxygen with sulphur, observing the ratio of 1, 2, 3, 4, and four proportions of hydrogen with carbon in the same ratio, so there will be found equal reason to infer the existence of four similar proportions of oxygen with carbon, 100 of carbon being combined in the first with 62.5 of oxygen, in the second with 125 constituting carbonic oxide, in the third with 187.5, and in the fourth with 250 constituting carbonic acid. With these preliminary observations it is sufficient to give the general results.

Citric acid appears to be carbon with oxygen in the third definite proportion, that between carbonic oxide and carbonic acid; and its hydrogen is nearly in the first proportion of that element to carbon.

Acetic acid is carbon with oxygen in the second proportion nearly, and with hydrogen in exactly the second proportion, that of 100 to 18. It is represented, therefore, by carbonic oxide, with hydrogen in the proportion which constitutes supercarburetted hydrogen.

Gallic acid is carbon with oxygen in none of the four definite

proportions, but almost exactly in the mean proportion between the first and second. Its hydrogen is nearly in the first proportion of that element to carbon.

Succinic acid is carbon with oxygen in the second proportion, that constituting carbonic oxide. The hydrogen conforms to none of the four proportions, but is the precise mean between the first and second.

In saccho-lactic acid the relation of the oxygen to the carbon is not that of any of the definite proportions, but is nearest to the third. The hydrogen is that which constitutes supercarburated hydrogen.

The analysis of benzoic acid is evidently very doubtful, owing to the difficulties which attend it from its volatility. It is the only one in which the proportion of oxygen to carbon is less even than the lowest of the definite proportions of these elements. The proportion of hydrogen is almost exactly that of the first proportion.

If the definite proportions of oxygen and hydrogen to carbon be assumed to be more numerous than four, but still observing the law of simple multiples, all these results may be easily brought under the law. The relations suggested by these researches, and particularly those which prove that proportions of carbon both to oxygen and to hydrogen exist inferior to the lowest known proportions of these elements, afford much support to the conclusion, that their definite combinations are more numerous than the few that have been admitted, either on the doctrine of equivalents, or on the atomic hypothesis. And on the latter, the composition of organic compounds may be accounted for with this conclusion, so as to preserve what constitutes its chief excellence—the principle that one body in a combination is always in the relation of one atom, and which is confessedly incapable of being maintained, with the assumption merely of the few definite proportions of the elements that have hitherto been assigned.

The view indeed that the vegetable acids are compounds of a simple radical (carbon) acidified by oxygen and hydrogen, and the law existing in this and other ternary combinations, that two of the elements observe the requisite relations in proportion to the third as a base, may probably be extended to all the vegetable, and, perhaps, even to the more complicated animal products; and, with the admission of a more extensive series of definite proportions in the primary elements, may remove the necessity of the law advanced by Berzelius, and apparently now admitted by the supporters of the atomic system, that while in inorganic bodies one of the constituents is always in the state of a single atom, in organic bodies it is not so, but very often the reverse. If this law be excluded, and the reverse established, it will assimilate the constitution of organic to that of inorganic com-





oxide and nitric acid, which, from their proportions, afforded the intermediate multiples 3, 4, that of real nitric acid being considered as 5. But Dulong has shown that these acids are the same. He has also obtained nitrous acid in its insulated state; its composition is 10 of nitrogen with 22.8 of oxygen—a proportion of oxygen which gives the multiple 4; so that the series is still incomplete, being that of 1, 2, 4, and either 5, or 7.

When this acid is acted on by an alkaline base, it is decomposed; one part passes to the state of nitric acid, and forms a nitrate; and the other forms a nitrite. It might be supposed, therefore, that one portion of it yields oxygen to the other, and that thus a subnitrous acid is formed, which might afford the intermediate proportion. Nitric oxide gas, however, is disengaged, so that there is probably no reduction in the degree of oxygenation. And if there were, it would, conformably to the principle illustrated under the consideration of sulphuric acid, be replaced by the oxygen of the base, and form the ternary compound constituting the nitrite, so that the relation of this element to the nitrogen would be the same. There is, therefore, no evidence of the existence of any definite compound intermediate between nitrous acid and nitric oxide, and the ratio of oxygen in nitrous oxide and these two compounds is that of 1, 2, 4.

The proportion in nitric acid, it has been stated, is that which gives the multiple 5 of oxygen. But this applies to what is called the real acid free from water, and no such compound exists, not even in combination with a base; for, as has been already shown, when an acid yields water from the action of a base, though there is thus an abstraction of a portion of its oxygen, it receives that of the base, and forms a ternary combination, in which the proportion of oxygen to the radical remains the same.

The real composition, therefore, must be determined in its state of hydronitric acid. The quantity of combined water, according to the common expression of the fact, existing in it has been variously stated; but if the estimate in Dr. Wollaston's scale of 0.25 in acid of the specific gravity 1.50 be taken, this gives as the composition 10 of nitrogen, with 40 of oxygen and 1.55 of hydrogen: and this again gives 7 as the multiple of oxygen in the series of compounds—a result which it is scarcely possible to connect according to the established law with the multiple 4, in the lower compound, nitrous acid.

It is certain, however, independent of this circumstance, that the quantity of water (or of oxygen and hydrogen equivalent to it), thus assigned, is not the just proportion essential to the constitution of the acid; for the specific gravity 1.50 is not the highest at which it can be procured. It is obtained with certainty at 1.55 at 60°; by some chemists it is stated at 1.58, and by Proust even at 1.62. At 1.50, therefore, it must be diluted with a certain portion in addition to the real combined water of

the common hypothesis. Dr. Wollaston has observed, that to decompose nitrate of potash so as to afford nitric acid, it is necessary to employ as much sulphuric acid as forms bisulphate of potash; and hence each portion of potash from which dry nitric acid is separated will displace the water from two equivalents of sulphuric acid. One of these portions of water, it may be presumed then, will go as essential to the constitution of the nitric acid, or rather its oxygen and hydrogen will do so, the other is adventitious, though from the volatility and facility of decomposition of the acid it may not be easily abstracted.

On this view, the composition of the acid will be found to be 100 of nitrogen, 34 of oxygen, and 0.76 of hydrogen, which gives 6 as the multiple of oxygen to the first proportion of that element. The proportion of hydrogen is to the nitrogen as the first or lowest equivalent, that in ammonia being the third, the former being 0.76 to 10, the latter to the same quantity of nitrogen 2.3.

The same view of the composition of hydronitric acid may be inferred from the proportion of oxygen and nitrogen in the dry nitrates. In these, as in other analogous cases, the abstraction of oxygen in the formation of water at their formation is compensated by the oxygen of the base; the metallic radical of the latter merely replaces the hydrogen of the acid, and the proportion of oxygen to the radical of the acid remains the same.

It thus appears, that the series of the nitrous compounds is nitrous oxide, nitric oxide, nitrous acid, and nitric acid. The oxygen in the first is to the nitrogen as 5.7 to 10; and taking this first proportion of oxygen as 1, that in nitric oxide is 2, in nitrous acid 4, and in hydronitric acid 6—a ratio sufficiently conformable to the law of definite proportions.

If it were admitted that the oxygen and nitrogen remaining after the action of hydronitric acid, and anhydrous nitrous acid, formed binary compounds, which entered into direct combination with the alkali; then from the abstraction of one proportion of oxygen in the one by the formation of water, and in the other by the production of nitric acid, compounds would be formed, intermediate in the former between hydronitric and nitrous acid, and in the latter between nitrous acid and nitric oxide, and thus the series of the proportions of oxygen of 1, 2, 3, 4, 5, 6, would be completed. This view, however, is not probable. At the same time, the relation of these elements in these intermediate proportions may exist in other ternary compounds, though they are not found in binary combination, or in the ternary combinations which they form with hydrogen, or with metallic bases.

(*To be continued.*)

## ARTICLE X.

*On Ferro-chyazate of Potash, and on the Atomic Weight for Iron.*  
By R. Porrett, Jun.

IN a paper of mine published in vol. xii. of the *Annals of Philosophy* for 1818, p. 214, on the Triple Prussiate of Potash, I drew a comparison between my analysis of this salt made in 1814 and that made by Dr. Thomson, which had just appeared, and after remarking, as that acute chemist had done, that the results did not accord with the atomic theory, I related an experiment I had made by decomposing the triple prussiate with tartaric acid, which seemed to prove that both had overrated the quantity of potash in that salt, and that when this was corrected, the number for its acid constituent would be represented on the scale of equivalents by 85·9. I showed also that a nearly similar number, namely 84·7, resulted from my analysis of ferro-chyazate of barytes (after rectifying a mistake in the quantity of water which I had attributed to that salt); and that if we considered the ferro-chyazic acid as composed of

4 atoms carbon . . . . .	30·16
1 atom azote . . . . .	17·54
1 atom iron . . . . .	34·50
2 atoms hydrogen. . . . .	2·64
	<hr/>
	84·84

the number representing its atomic constitution would then agree very well with the equivalent number derived from the two analyses above-mentioned. I, therefore, proposed this view of its nature as a probable explanation of the discrepancies between the former experimental results, and those deducible from the application of the atomic theory. But this explanation, hitherto only probable, assumed all the appearance of a well-established fact; when upon carefully collecting and examining the gases produced from the combustion of ferro-chyazic acid, I actually obtained carbonic acid and azote gases in the proportions of four volumes of the former to one volume of the latter gas; and in quantities which, as nearly as could be expected, were the same as calculation indicated.

After this I entertained no doubt but that I had given the exact composition of ferro-chyazic acid, and also of its salts, with base of potash and of barytes; and was very much surprised when I learned that Mr. R. Phillips had ascertained that it did not contain so much iron as I had assigned to it. This circumstance Mr. Phillips obligingly requested a mutual friend to communicate to me.

My attention being thus again called to the subject, I soon

perceived that in the formula which I had proposed as representing the atomic constitution of the ferro-chyazate of potash, the only part which did not appear to be supported by the results of experiment, was that which gave the quantity of iron as amounting to one atom, all my experiments, as well as those of Dr. Thomson, having made it less; and Mr. Phillips's message seemed to prove that these experimental results were much nearer to the truth than the theoretical ones.

I, therefore, felt the necessity of making further researches into the very complicated composition of this salt, convinced that the uncommon difficulties which its analysis presents, had never yet been so completely overcome as to admit of its constituents being stated with that rigid accuracy which is now so essentially required in chemical investigations.

I began with experiments to ascertain the quantities of iron and of potash which the salt contains; and after trying various modes of analysis, I fixed upon the following, as being susceptible of greater accuracy than any other which I had attempted.

Twenty-five grains of ferro-chyazate of potash were burned in a covered platina crucible with nitrate of ammonia; after the combustion, which was very vivid, I found in the crucible a saline mass mixed with red oxide of iron, which weighed 21.5 gr. and was composed of 17 gr. of nitrite of potash, and 4.5 gr. of red oxide of iron: I separated the nitrite of potash from the oxide of iron by lixiviation, and added a sufficient quantity of muriatic acid to it to convert it into a muriate; nitrous gas was immediately given off; and after evaporating to dryness, I obtained 16.5 gr. of chloride of potassium; these quantities of red oxide of iron and of chloride of potassium are equivalent to 10.42 potash, and 3.15 iron, which, multiplied by 4, give 41.68 of the former, and 12.6 of the latter substance, as contained in 100 of the ferro-chyazate.

The quantity of alkaline base in the salt being thus ascertained, and also the ferruginous portion of the acid, I had next to find the quantities of the other constituents of that acid; namely, its carbon, azote, and hydrogen; and for this purpose, I repeated several times the combustion of the salt, mixed with peroxide of copper: the average quantity of gases collected from one grain of the salt, with from 20 to 30 times its weight of peroxide, was as follows:

	Cubic inches.	Volumes.
Carbonic acid .....	1.79 .....	4
Azote .....	0.45 .....	1
	<hr/> 2.24	<hr/> 5

a result which exactly corresponds as to the proportions between the two gases with what I before published, but which differs



from it by an excess of nearly half a cubic inch in their total quantity; the true quantity, I believe, lies just half way between them; that is to say, it should be precisely two cubic inches; nevertheless I shall not here make any corrections in the experimental quantities above obtained, but draw my conclusions strictly from them, leaving it ultimately to the atomic theory to polish off the rough product of experiments; taking, therefore, 1.79 cubic inches of carbonic acid gas as representing 0.2264 of a grain of carbon, and 0.45 of a cubic inch of azote gas as equal to 0.1332 of a grain of azote, we obtain the numbers 22.64 and 13.32 for the quantities of carbon and of azote in 100 of the salt in question.

In the experiments made by burning the salt with peroxide of copper, I was for some time puzzled to account for the necessity of employing so much of the oxide as I found requisite in order to obtain the maximum quantity of gases; but I subsequently discovered that the oxide which I employed contained a minute quantity of silica; and that when a sufficiency of this last was not introduced by means of the oxide, the potash of the salt retained behind with it a portion of carbonic acid, which, in the opposite case, was expelled as gas when the silica combined with that alkali at a red heat. Hence it would seem adviseable, in all future attempts, to ascertain the composition of a combustible acid by burning it united to an alkaline base, to add to the peroxide of copper employed a proportion of silica sufficient to saturate the alkali, and prevent its remaining behind in the state of subcarbonate, or of nitrite, by converting it into a silicate.

I estimated the quantity of hydrogen by ascertaining that of the peroxide decomposed in the above experiments beyond what was employed in converting the carbon into carbonic acid. My mode of doing this was to act upon the residuum of the combustion by diluted sulphuric acid, which dissolved all the oxide that had not been decomposed, and left the remainder in the metallic state. From the weight of this reduced copper, it was easy to infer that of the oxygen expended, and by subtracting therefrom what combined with the carbon, to deduce the proportion of hydrogen which united with the residue. The quantity of hydrogen which, by this means, I computed to exist in 100 gr. of the salt was 0.8 of a grain.

I may here remark, that this mode of computing the hydrogen appears to be susceptible of greater precision than that of weighing the water which may be collected after the experiment; for to say nothing of the difficulty of such collection, it must sometimes be a matter of uncertainty whether part of that water did not exist as such in the substance operated upon; besides, one part of hydrogen furnishes only nine times its weight of water, but it decomposes 40 times its weight of peroxide of copper. A minute quantity of hydrogen, therefore, is more readily and





certainly detected by weighing the metal reduced by its means than by weighing the water which it produces : the only circumstance which it is necessary to guard against, when this process is resorted to, is the oxidation of any portion of the azote.

To return to the analysis.—It now only remained that I should ascertain the quantity of water of crystallization in the salt; but renewed experiments to determine this added nothing to what I had before stated, and which had been confirmed by Dr. Thomson; namely, that only 13 per cent. could be separated from it at a temperature below that which would decompose its acid. In taking 13 then as the quantity of water in 100 of the salt, we cannot be certain that it indicates the whole quantity, but must rest satisfied with it as the nearest approximation to that quantity which experiment is capable of giving us.

Collecting now, from the preceding experiments, the proportions of all the constituents of 100 gr. of ferro-chyazate of potash, they appear as follows :

	Grains.
Potash .....	41·68
Ferro-chyazic acid {	Iron. .... 12·60
	Carbon ..... 22·64
	Azote. .... 13·32
	Hydrogen ..... 00·80
Water. ....	13·00
	<hr/> 104·04

being a surplus of four grains, arising from the unavoidable inaccuracies in determining experimentally on small portions of the salt the proportions of so many constituents.

These inaccuracies are easily removed by the application of the atomic theory; for by taking as our guide the weights of the atoms of each of the elements, we obtain the following numbers :

1 atom potash .....	= 60·00	.... 40·34
1 atom ferro-chyazic acid = 66·25 {	$\frac{1}{2}$ atom iron .....	= 17·50 .... 11·76
	4 atoms carbon. ..	= 30·00 .... 20·17
	1 atom azote. ....	= 17·50 .... 11·76
	1 atom hydrogen ..	= 1·25 .... 00·84
2 atoms water. ....	= 22·50	.... 15·13
1 ferro-chyazate of potash .....	= 148·75	.... 100·00

which doubtless gives the true proportions of the several elements of this salt.

Thus it appears that in attempting to reconcile the analysis of ferro-chyazate of potash with the numbers generally received as representing the relative weights of the atoms of its several elements, we are reduced to the necessity of considering one of

these elements; namely, the iron, as half an atom only; and this palpable absurdity can only be obviated by one of the two following methods: that is, either by multiplying all the atoms by 2, so as to get rid of the fraction, or by considering the number hitherto admitted for the atom of iron as being double what it ought to be; but by the former of these methods, we should obtain a number representing the weight of an atom of ferro-chyazate of potash, which would not be equivalent to the known quantities of the different chemical agents which decompose it. Such a number, therefore, must be wrong, and the method of producing it must be so likewise. The other method I have, therefore, no hesitation in resorting to; and on trying how far the number thus deduced for the atom of iron (namely 17.5) could be reconciled with the well-ascertained composition of its oxides, sulphurets, and chlorides, I had the great satisfaction of finding that it was easily reconcileable to them; and, moreover, that it was the only weight hitherto deduced for iron which was reconcileable to them *all*; the others being so only partially. Thus although the number 35 for iron with 10 for oxygen = 45 exactly expresses the composition of protoxide of iron, yet it does not adapt itself to that of the peroxide, which to 35 iron contains 15 of oxygen, or  $1\frac{1}{2}$  atom of the latter: here the absurdity of half an atom shows itself. The difficulty in this case was apparently well got over by Dr. Thomson, who considered the peroxide as composed of two atoms of iron with three atoms of oxygen, or as  $70 + 30$ ; and if there were only two oxides of iron, this explanation would very well apply. But there is an oxide of iron distinctly established both as existing in nature and as a product of art (I mean that which Berzelius has called the oxydum ferroso ferricum, and which Gay-Lussac obtained artificially by passing the vapour of water over red-hot iron), which contains  $27\frac{1}{3}$  per cent. of oxygen—a proportion intervening between that in the protoxide and that in the peroxide, the composition of which cannot be stated in whole atoms either by taking the weight of iron as 35, or as 70: with the former it must be set down as with 13.3, or  $1\frac{1}{3}$  oxygen, and with the latter as with 26.6, or  $2\frac{2}{3}$  oxygen. The attempt to consider this oxide as a combination of one atom protoxide with two atoms peroxide, instead of a distinct oxide of itself, can only be considered as one of those ingenious expedients which have been resorted to in order to make an experimental result harmonize with a theoretical one when the two appeared discordant; it will doubtless be abandoned now that it can be shown that this discordance was only apparent, and resulted from taking the weight of an atom of iron at double its real amount.

The following table of the compounds of iron with sulphur, oxygen, and chlorine, will show the manner in which the new number now proposed for iron adapts itself to all those compounds, several of which the whole number is inapplicable to.

IRON, 17.5.											
With Sulphur, 20.				With Oxygen, 10.				With Chlorine, 45.			
<i>Protosulphuret, or Magnetic Pyrites.</i>				<i>Protoxide, or Black Oxide (magnetic).</i>				<i>Protochloride.</i>			
2 Iron.....	35	63.75	100	2 Iron.....	35	78	100.0	2 Iron.....	35	43.75	100.0
1 Sulphur ..	20	36.25	57	1 Oxygen ...	10	22	28.5	1 Chlorine ..	45	56.25	128.7
	<u>55</u>	<u>100.00</u>	<u>157</u>		<u>45</u>	<u>100</u>	<u>128.5</u>		<u>80</u>	<u>100.00</u>	<u>228.7</u>
<i>Deutosulphuret.</i>				<i>Deutoxide (Gay-Lussac's) (slightly magnetic).</i>				} No corresponding chloride known.			
3 Iron .....	52.5	57	100	3 Iron.....	52.5	72.5	100				
2 Sulphur...	40.0	43	76	2 Oxygen..	20.0	27.5	38				
	<u>92.5</u>	<u>100</u>	<u>176</u>		<u>72.5</u>	<u>100.0</u>	<u>138</u>				
<i>Tritosulphuret.</i>				<i>Tritoxide, or Red Oxide.*</i>				<i>Perchloride.</i>			
4 Iron .....	70	54	100	4 Iron.....	70	70	100	4 Iron....	70	34.14	100.00
3 Sulphur. ....	60	46	86	3 Oxygen.....	30	30	43	3 Chlorine	135	65.86	192.85
	<u>130</u>	<u>100</u>	<u>186</u>		<u>100</u>	<u>100</u>	<u>143</u>		<u>205</u>	<u>100.00</u>	<u>292.85</u>
<i>Persulphuret, or Cubic Pyrites.</i>				} No corresponding oxide known.				} No corresponding chloride known.			
1 Iron. . .	17.5	46.5	100.0								
2 Sulphur	20.0	53.5	114.2								
	<u>37.5</u>	<u>100.0</u>	<u>214.2</u>								

In the first column of the above table, I have introduced two sulphurets not hitherto admitted by chemists, the existence of which it is, however, easy to infer from Proust's experiments, recorded in his paper on the Native and Artificial Sulphurets of Iron, a translation of which may be seen in the first volume of the octavo edition of Nicholson's Philosophical Journal. He distilled 400 gr. of fine cubic crystals of native persulphuret, and separated from it 75 gr. of sulphur: there remained a sulphuret weighing 322 gr.: hence 100 gr. would have become 81 gr. Now as 100 persulphuret contain 46.5 of iron, these 81 gr. must have contained the same quantity of that metal combined with 34.5 of sulphur; but as 46.5 is to 34.5, so is 57.4 to 42.6, which agrees very closely with the composition assigned in the table to the deutosulphuret.

In another experiment described by Proust, he succeeded by very cautiously applying a heat below redness to a mixture of protosulphuret and sulphur in making the former take up half as much sulphur again as it was previously combined with. Now on referring to the table, it will be immediately obvious that he must have formed the tritosulphuret which with 100 parts of iron

\* From Haüy's Experiments on Double Magnetism, it would seem that even the red oxide retains some magnetic influence.

exactly contains this increase of sulphur over that in the proto-sulphuret. Proust's experiments give the proportions of sulphur in these two sulphurets as 60 to 90 with 100 of the metal, numbers which differ but little from those in the table.

It is, therefore, evident, from the two experiments just described, that Proust had formed two new compounds, the deuto and the tritosulphurets, although he was not himself aware of it, but considered them as agreeing in composition with the two sulphurets commonly known as protosulphuret and as persulphuret.

From the preceding experiments and deductions, I think it will be clear that the atom of oxygen being 10, that of iron should be represented by the number 17·5, the same as that of azote, and that we are now entitled to consider the atom of ferro-chyazic acid as composed of

4 atoms of carbon. . . . .	= 30·00
1 atom of azote. . . . .	= 17·50
1 atom of iron . . . . .	= 17·50
1 atom of hydrogen. . . . .	= 1·25
	<hr/>
	66·25

also that this acid combines with one atom of base and two atoms of water to form the ferro-chyazates.

The reduced weight for an atom of iron will, I trust, account for the small proportion in which this metal and its oxides enter into the composition of several mineral bodies, and will, perhaps, show that in some of those instances where it has been considered as an accidental ingredient, it is in reality chemically united in atomic proportions with the other constituents of the mineral.

*Tower, Sept. 18, 1819.*

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## ARTICLE XI.

### ANALYSES OF BOOKS.

*A Critical Examination of the first Principles of Geology; in a Series of Essays.* By G. B. Greenough, President of the Geological Society, F.R.S. F.L.S. London.

THIS work deserves the particular attention of geologists. It comes from a gentleman who has been for many years enthusiastically devoted to geological pursuits; who has dedicated to them the greatest part of his time and study, and who has appropriated a very handsome fortune to the promotion of his favourite objects. He has read every thing that has been writ-





resulted from the rotation of a liquid globe round its axis with a velocity equal to that of the earth. This was demonstrated by Sir Isaac Newton, and has been amply confirmed by the subsequent labours of mathematicians and astronomers. The specific gravity of the earth may be considered as determined by the experiments of Maskelyne, and the subsequent calculations of Dr. Hutton and Mr. Playfair, and the delicate experiments of Mr. Cavendish, upon which I am disposed to place the greatest reliance. Now as the mean specific gravity of the crust of the earth cannot easily be rated higher than 2.5, it follows as a consequence that the specific gravity of the central parts of the globe must be much greater than 5. It must, therefore, be composed of solid matter of a metallic nature.

With respect to the nature of the fluid matter which constituted the original globe of the earth, the conjectures of geologists have been various and contradictory. The imagination, unrestrained by the present state of things and the present laws of matter, has been set at complete liberty to wander amidst a chaos of its own creation—

————— a dark  
 Illimitable ocean, without bound,  
 Without dimension, where length, breadth, and height,  
 And time, and place, are lost.

Bertrand was of opinion that the earth consisted originally of pure water, and that by the plastic power of nature, this water was gradually converted into the various earths and metallic bodies of which the globe is at present composed. This opinion, compared with which the reveries of the alchymists were modest and plausible, was advanced at the end of the eighteenth century, and by a man who considered himself as profoundly skilled in all the physical discoveries that preceded him. Some circumstances lead me to suspect that an opinion not very dissimilar was embraced by Werner, or at least by some of the most eminent of his pupils. They nowhere indeed state it in express terms, but they talk of the gradual diminution of water; and I have been told by some of them, that this liquid would ultimately fail altogether; or that there was a slow change of this globe from a state of liquidity to a state of solidity—a change which had been going on from the origin of things, and was still in progress. This I consider as tantamount to Bertrand's notion, that the primordial state of the globe was pure water.

But the most general opinion entertained by geologists is, that all the substances which at present exist in the globe constituted a part of it from the beginning; but that they were at first in a liquid state, and gradually deposited from the watery portion, which still retains its liquidity, partly by crystallization, and partly mechanically.

From the phenomena of crystallization duly witnessed in the manufactories of the different salts, it is obvious that when solids





the two cases. The same diversity of opinion exists with respect to the granite rocks in our own country. Prof. Jameson describes Goatfield as stratified, Prof. Playfair as unstratified. Prof. Playfair says, that Mount Sorrel is stratified, while Lord Webb Seymour affirms that it is unstratified.

The same diversity of opinion exists with respect to the stratification of sienite, porphyry, hornblende, greenstone, serpentine, transition limestone, siliceous slate, and greywacke.

Our author endeavours to account for this diversity of opinion by the different ideas which different writers affix to the word, or by appearances from which they do not deduce the same conclusions. These are chiefly the following:

1. Some rocks present external planes parallel to each other; but are not subdivided by internal planes. I presume that the author means, that in some cases when we survey the face of a rock, we observe lines parallel to each other at certain intervals, indicating at first sight that the rock is divided into different layers or beds; but upon examining these lines, we find them to be merely superficial, and not the terminations of planes penetrating through the body of the rocks, and dividing it into different beds. When this occurs, as is sometimes the case, it is obvious that an observer may be easily so far misled as to ascribe stratification to rocks which do not possess it.

2. Parallel internal planes are not always coextensive with the rock in which they occur; that is to say, when we survey the face of a rock, we sometimes observe parallel lines, which are the terminations of planes dividing the rock into layers; but these planes do not penetrate through the whole rocks, but terminate at unequal distances from the exposed surface; so that the rock appears partly stratified, partly unstratified. In such a case, the stratification must be illusory.

3. Parallel planes are sometimes too distant from each other, and sometimes too near each other, to give the rock in which they occur an undisputed claim to stratification. This obviously depends upon the meaning affixed to the term *stratification*. If it signifies deposition from a liquid, the restriction is correct. When it is said that Mont Blanc consists of three vertical beds of granite, it must be obvious that these beds could never have been mechanically deposited from the sea. In the same way, though mica-slate and clay-slate are composed of flakes lying above each other, yet this slaty structure is not of itself sufficient to demonstrate that these rocks were mechanically deposited from the sea.

4. Two or more sets of parallel planes sometimes occur in the same rock, so as to meet perpendicularly or obliquely. This appearance, as far as I have observed it, occurs in limestone and gypsum, and seems to depend upon crystallization.

5. In rocks that have the appearance of deposition, the planes of the strata are not always parallel. We should expect this to

be the case when matter is deposited upon an inclined plane. The bed ought to be thickest at the bottom, and thinnest at the summit of the inclined plane on which it is deposited. This sometimes happens, and indicates obviously the cause from which it results.

6. The thickness of masses often varies considerably in different parts of their course. This is well known to be the case with the coal beds.

7. The term *stratification* is connected with theory. Many persons think that rocks are not stratified unless they have been deposited from a fluid menstruum. They conceive that the strata are produced by depositions alternately suspended and renewed. But our author considers it as certain that this is not always the case for the following reasons :

(1.) Parallelism of surface may be produced by other causes, as in basalt, tabular flint, &c.

(2.) The recurrence of parallel planes depends upon the substances deposited. Granite, porphyry, trap, salt, chalk, are always in thick masses; argillite in flakes; sandstone and oolite in beds.

(3.) The larger divisions of rocks are frequently not parallel to the laminae of which these rocks are composed.

(4.) Way-boards depend upon the nature of the rock : stony beds have them; while sand-clays, loams, &c. are destitute of them.

(5.) At the junction of two kinds of rock, as greywacke slate and limestone, we find that each is impregnated with the substance of the other.

(6.) The contemporaneous veins of one stratum sometimes extend themselves into the adjoining stratum.

(7.) Decomposition or torrefaction often expose to view stratification which was before latent.

(8.) Depositions which go on uninterruptedly in our laboratories frequently arrange themselves in distinct layers.

These reasons in order to produce conviction would require to be much more developed than the author has thought requisite. Indeed I must acknowledge that I do not perceive the force of some of them, nor the truth of others. I should like to know, for example, what uninterrupted depositions in chemical laboratories take place in distinct layers. I am not aware of any example of this in crystallizations. Mechanical deposits doubtless arrange themselves in layers when they differ considerably in their specific gravities; but such depositions are not entitled to the name of chemical. They are entirely owing to the action of gravity, and accordingly follow the order of their specific gravity. Geologists have not attended sufficiently to the difference between crystallization and mechanical deposition. When the Wernerians say that granite is stratified, and at the same time that it was separated from a fluid by crystallization, I am of opinion that they are guilty of a contradiction in terms.

Attention to this distinction will enable us likewise to appreciate the value of some late notions that have emanated from the same school, as that sandstone, traff-tuff, greywacke, &c. are chemical deposits.

Almost every kind of rock occurs both in a horizontal and a vertical position. Now it has been very much agitated among geologists, whether the vertical position of rocks was original, or the consequence of some catastrophe subsequent to their creation. Our author states the arguments on both sides with much coolness and candour. The arguments to show that the strata were originally horizontal are as follows :

1. In some cases we observe the same strata on the one side of a fault horizontal, while on the other side they are inclined. Now in such cases it is infinitely probable that both were originally horizontal, and that this position was deranged on the one side by the fault. This argument, therefore, is conclusive as far as it goes.

2. From the nature of the materials of which they are composed, some strata, though at present vertical, must have been originally horizontal. Thus it is impossible to suppose that the vertical strata of loose sand that occur at Alum Bay were originally deposited in that state.

3. Inclined strata have sometimes the undulations on their surface, which we meet with on a sandy beach.

4. If the preceding arguments be admitted as conclusive, we must go a step further ; for these beds are often so connected with others below them that we cannot say where the series ends.

These arguments seem sufficient to prove, that there are some beds at present vertical or inclined that were originally horizontal ; but I do not see how they give any countenance to the notion that *all* vertical beds were originally horizontal.

The arguments to show that the strata have remained unaltered are the following :

1. It appears from veins that the particles of matter may arrange themselves in beds highly inclined to the horizon.

2. Inclined strata are often unaccompanied by marks of violence, and exhibit the greatest regularity both in form and direction.

3. Vertical or inclined strata are often mantle shaped.

4. The rocks on the two sides of a mountain chain, as the Alps, are often different.

5. The irregularities in the strata seem to warrant that they are in their original position. Thus at Malvern the inclination of the sandstone diminishes as it recedes from the hill, and then increases. To this succeed strata of limestone, the inclination of which becomes more and more considerable.

6. Secondary rocks are generally inclined at their junction with primary.

7. The secondary rocks are often unconformable to the primary rocks on which they rest.

If beds were originally horizontal, and afterwards shifted, the supposable causes of this shift are: 1. An internal force acting from below upwards so as to raise the crust of the globe. 2. A want of support, owing to internal cavities, so that the beds have fallen by their own gravity. 3. An external shock, which has broken the shell, and made one part tumble over another. Dolomieu was inclined to the last of these opinions; but if the inclination be original, to what circumstances can we ascribe it? The following are the explanations that have been attempted:

1. Hutchinson ascribes the inclination of slate to the shape of the component particles.

2. Many authors ascribe the inclination of the strata to the inequality of the base on which they were deposited.

3. Werner considered the primary rocks to be crystalline and stratified; the transition rocks to be partly crystalline, partly mechanical; and the secondary as mechanical.

There are many beds of rocks which are subject to curvature and angularity. Gneiss, mica slate, chlorite slate, clay slate, old red sandstone, calp, limestone, and alluvial sand, may be mentioned as well-known examples.

These curves are sometimes exceedingly large, sometimes exceedingly small; and very often they constitute curves of double curvature. These curves have been ascribed to the following causes:

1. To the strata being lifted while flexible and ductile. But this explanation cannot be admitted, because curved strata often lie upon horizontal strata.

2. To crystallization. But the curvature of contiguous strata composed of different materials seems fatal to this opinion.

Our author thinks that the curvature of strata is owing to three different causes. 1. To the unequal effects produced by temperature on the materials. 2. To the motion of the fluid from which they were deposited. 3. To the shape of the ground on which they were deposited.

Mr. Greenough terminates this essay as follows:

“To conclude, then; let me ask, where a rock is *stratified*; is it necessarily bounded by parallel surfaces? If so, let us hear no more of mantle-shaped, saddle-shaped, shield-shaped, fan-shaped, basin-shaped, trough-shaped stratification.

“Are its surfaces necessarily parallel to those of the adjoining rock? If so, let us hear no more of unconformable and overlying stratification.

“Is it sufficient that parallelism shall be found in a portion of the rock? Let us never hear of substances being unstratified. Or must it extend through the entire mass? Let us hear no more of strata.

“The laminæ of flagstone, the folia of slate, are these strata? Are laminæ, 400 yards thick, strata? Is there any assignable limit to their thickness or tenuity?



"When one set of parallel planes crosses another, are both sets to be called strata, or neither, or only one of them? If one only, by what rule are we to be guided in distinguishing the real from the counterfeit?"

"Must the beds be so arranged as to convey to the observer the idea of deposition alternately suspended and renewed? If this is not necessary, how is the parallelism derived from stratification to be distinguished from parallelism resulting from other causes? and of what use is it to know whether a substance is stratified or not? If it is necessary, where two observers have imbibed contrary impressions, how shall we determine which of the two is right?"

"Let him who can answer these questions rest assured that he has a distinct idea of stratification."

(*To be continued.*)

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## ARTICLE XII.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Death of Mr. Watt.*

James Watt, Esq. the celebrated improver of the steam-engine, died at his house at Heathfield, near Birmingham, on Aug. 25, in the 84th year of his age. He had complained of indisposition during his usual summer visit to London. This was attributed to the effects of an inveterate cough, but he did not lay aside his intention of making an excursion to Scotland. Some time after, he was seized with an affection of the liver, from which he partly recovered; but the nausea and indigestion could not be wholly removed; and nature at his advanced age proved unequal to the struggle. He himself seems to have been aware from the first of the fatal tendency of his complaints; but he looked to the event with calm tranquillity, and retained possession of his faculties to within a few hours of his death.

We trust to have it in our power very soon to lay a biographical account of this great ornament of our age before our readers. In the meanwhile, we shall close the present article with the following paragraph, copied from the *Birmingham Gazette* of Aug. 30, 1819:

"By the death of this truly great man our country is deprived of one of its most illustrious ornaments. Mr. Watt may justly be placed at the very head of those philosophers who have improved the condition of mankind by the application of science to the practical purposes of life." His steam-engine is probably the most perfect production of physical and mechanical skill



which the world has yet seen; while in the variety, extent, and importance of its applications, it certainly far transcends every similar invention. So great was the activity and power of his mind, that he not only embraced the whole compass of science, but was deeply learned in many departments of literature: and such was the felicity of his memory, that it retained, without effort, all that was confided to it. He was still more distinguished, not only by that highest prerogative of genius, promptness and fertility of invention, but also by its rare and happy union with a calm and sagacious judgment, regulated and matured by those habits of patient attention and investigation, without which no great production of human art was ever carried to perfection. His manners were marked by the simplicity which generally characterizes exalted merit; he was perfectly free from parade and affectation; and though he could not be unconscious either of the eminent rank he held among men of science, or of those powers of mind by which he had attained it, yet his character was not debased by the slightest taint of vanity or pride. He had for many years retired from business, but his mind continued actively employed on scientific improvements. He perfected an apparatus for the medical application of factitious airs; and the amusement of his latter days was the contrivance of a machine for imitating and multiplying statuary, which he brought to a considerable state of perfection. Happy in his domestic connexions, in the complete enjoyment of his extraordinary intellect, respected and beloved by the wise and good of every country; and having attained the great age of 84 years, his useful and honourable life was terminated, after an illness of short duration, rather of debility than of pain, by an easy and tranquil death.

“Mr. Watt was elected a Fellow of the Royal Society of Edinburgh in 1784; of the Royal Society of London in 1785; and a Member of the Batavian Society in 1787. In 1806, the honorary degree of Doctor of Laws was conferred upon him by the spontaneous and unanimous vote of the Senate of the University of Glasgow; and in 1808 he was elected a Member of the National Institute of France.”

## II. *Professor Playfair.*

In the number of the *Annals of Philosophy* for August last, there was inserted a short notice of the death of this eminent scientific character, containing the very erroneous statement that he was the son of Dr. James Playfair, author of a *System of Chronology*, and ascribing to him the system of geography, known by the name of Playfair's Geography. That notice was inserted without the knowledge of the editor, who had the honour of being personally acquainted both with Professor Playfair, of Edinburgh, and with Principal James Playfair, of St. Andrew's, and would not have fallen into the mistakes

respecting them contained in the notice inserted in the August number of the *Annals*.\*

Dr. James Playfair, Principal of St. Andrew's College, and the author of the *Chronology*, and the *Geography*, was about 10 years older than Prof. Playfair, of Edinburgh; of whom he was a distant relation, a cousin, twice or three times removed. Mr. John Playfair, Professor of Natural Philosophy in the University of Edinburgh, was the son of the Minister of Liff in the carse of Goury. He succeeded him as clergyman of the same parish, when scarcely more than 22 years of age, and had the support of his mother, and brothers, and sisters, devolved upon him. He continued in the situation of a clergyman about two years, when he was employed by the late Mr. Ferguson, of Reith, as tutor to his son, with an annuity, equivalent to his stipend as minister of Liff. In this situation he continued also about two years, when, chiefly by the influence of Prof. Dugald Stewart, he succeeded him as Professor of Mathematics in the University of Edinburgh. His first publication, so far as I know, was a paper on impossible quantities inserted in the *Phil. Trans.* for 1778. As he is there styled the Rev. John Playfair, a designation which he dropped as soon as he left his living, I presume that when he wrote it he was minister of Liff. His writings, a few mathematical and biographical papers excepted, to be found in the *Transactions of the Royal Society of Edinburgh*, were chiefly controversial, partly on the Indian astronomy, and partly on the Huttonian theory. The very high reputation which he enjoyed was not owing to his writings, though they possessed uncommon energy, and were distinguished by their arrangement and their clearness, nearly so much as to his powers of conversation, which were uncommonly great, and to his manners, which were polished and exceedingly engaging. He was a whig of the new school, and a very zealous politician. But his treatment of his mother and brethren, who were left dependant on him, and whom he supported with the utmost tenderness and generosity, even when he could not very well afford it, demonstrates, I think, the goodness of his heart, and the soundness of his principles at bottom.

### III. *Method of rendering Glass less Brittle.*

An American gentleman has transmitted to the Editors of the *Annales de Chimie et de Physique* (vol. ix. p. 422), the following method of rendering glass capable of bearing sudden changes of temperature without breaking. Let the glass vessel be put into a vessel of cold water, and let this water be heated boiling-hot, and then allowed to cool slowly of itself without taking out the glass. Glasses treated in this way may, while cold, be suddenly filled with boiling-hot water without any risk of their

\* It was incautiously copied from one of the daily newspapers, and should have been confined to the simple statement of the Professor's lamented decease, reserving a more detailed account of him for a future number.—PUBLISHER.

cracking. The gentleman who communicates the method says, that he has often cooled such glass to the temperature of  $10^{\circ}$ , and poured boiling water into them without experiencing any inconvenience from the suddenness of the change.

If the glasses are to be exposed to a higher temperature than that of boiling water, he proposes boiling them in oil.

IV. *On the different Quantities of Rain collected in Rain-Gauges at different Heights.* By Mr. Meickle.

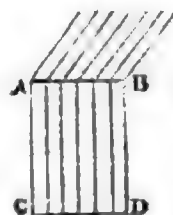
(To Dr. Thomson.)

SIR,

35, Berner's-street, Aug. 25, 1819.

In the *Annals of Philosophy* for August, I perceive a very superficial attempt to account for the difference observed in the results of rain-gauges placed at the top and bottom of a building. The author, M. Flaugergues, with the utmost certainty, ascribes it to the wind's altering the general direction of the rain; and seems, with all due solemnity, to demonstrate, by means of a diagram too, that "the quantity of rain which enters the rain-gauge is proportional to the sine of the inclination of the rain."

This explanation is no doubt very satisfactory to that class of readers who rest all on authorities, and never think for themselves. But the slightest reflection is sufficient to lay it aside altogether. It is easy to see, that the *horizontal* distance of the lines in which the rain falls is absolutely independent of their inclination, being accurately the same where the wind runs steadily 60 miles an hour, as if it were a perfect calm. By merely looking at the figure, it is plain that a gauge of the width, A B, will there receive the drops falling obliquely, just the same as after they become perpendicular in the calm at C D.\*



Such an explanation, therefore, as that of M. Flaugergues, has no concern with the case under consideration. Nothing indeed can be more vague than some conclusions which he draws from his doctrine, respecting the proper position of a rain-gauge. Were his opinion really correct, the quantity of rain which falls in any place would depend chiefly on the state of the wind. The very injudicious way in which he has drawn his diagram is sufficient to lead thousands into the same mistake.

I can hardly pretend to give a complete solution of this well-known paradox; but am disposed to think it is some way owing to the obstruction which the gauge itself offers to the wind. Perhaps the wind's being made to rush with greater rapidity, and a little upward in beginning to pass over the mouth of the gauge, prevents the rain from falling into that part of it which is next the wind. A sudden or abrupt increase of the wind's

\* In strictness, the drops fall in curves, but this is sufficient to show the truth of the above observation.

velocity would certainly augment the horizontal distance of the lines in which the drops fall, although only whilst and where the velocity is increasing. Now we have reason to believe that such an increase does take place over the mouth of the gauge.

I am, Sir, your most obedient servant,

HENRY MEIKLE

*V. Meteorological Observations at Cork.* By T. Holt, Esq.  
(With a Plate. See XCVII.)

(To Dr. Thomson.)

SIR,

Cork, April 3, 1819.

I TRANSMIT you the meteorological scale and journal for Cork, for the first quarter of 1819; and am, Sir,

With due respect, your obedient humble servant,  
THOMAS HOLT.

REMARKS.

JANUARY.

1. Foggy morning; bright day.
2. Fair; cloudy; rainy night.
3. Cloudy; high wind.
4. Showery.
5. Showery.
6. Showery; heavy rain; gale.
7. Bright.
8. Fair; heavy rain and wind.
9. Showery; rain.
10. Rainy; windy.
11. Showery.
12. Dry; cloudy; breeze.
13. Bright.
14. Misty; dry; cloudy.
15. Dry; cloudy.
16. Misty; rain.
17. Showery; gale.
18. Fair; showery.
19. Ditto; ditto.
- 20, 21, 22. Showery.
- 23, 24. Snow showers.
25. Snow; heavy rain; hard frost.
26. Frost; thick fog.
27. Fair.
28. Showery.
29. Fair; showery.
30. Showery.
31. Bright.

FEBRUARY.

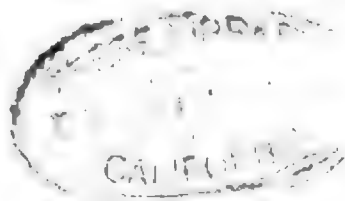
1. Fog; fair; rain.
2. Fair; frost.
3. Bright; showery.
4. Ditto; ditto; fog.
5. Rain last night; fine day.
6. Ditto; showery.
7. Bright.

8. Fine; showery.
9. Rainy.
10. Bright; breeze.
- 11, 12. Fair; occasional showers.
13. Bright.
14. Misty.
15. Rainy.
16. Showery.
17. Bright; occasional showers.
18. Rainy; heavy showers.
19. Rainy night; occasional showers.
20. Frost; rainy.
21. Fair; showers; gale.
22. Frost; fair; misty evening.
23. Showery.
24. Bright; light showers; gale.
25. Bright; misty evening.
- 26, 27. Bright; occasional showers.\*
28. Bright.

MARCH.

1. Bright; breeze.
2. Snow; frost; fair.
3. Frost; fair; light snow showers.
- 4, 5, 6. Bright; breeze.
- 7, 8. Dry; cloudy.
- 9, 10, 11, 12, 13, 14. Bright days.
15. Rainy morning; fine.
16. Fine; occasional showers.
17. Bright.
18. Cloudy; showers.
- 19, 20. Gale; fine; showers.
21. Misty; bright.
- 22, 23, 24, 25, 26, 27. Bright; occasional showers.
28. Rainy.
- 29, 30. Fair; showers.
31. Showery.

\* Feb. 27, about five, p. m. five columns of cloud, resembling water-spouts, but connected laterally, appeared in the S.E. apparently about three miles from Cork,





## RAIN.

1819.	Inches.	1819.	Inches.	1819.	Inches.
Jan. 3	0·124	Feb. 1	0·318	Mar. 2	0·150
4	0·630	3	0·030	15	0·036
5	0·372	4	0·138	16	0·018
6	0·390	5	0·048	18	0·024
8	0·420	6	0·042	20	0·042
9	0·906	9	0·078	27	0·348
10	0·234	12	0·408	28	0·416
11	0·180	15	0·702	30	0·414
16	0·030	17	0·228	31	0·066
17	0·145	18	0·348		
18	0·230	20	0·210		1·514
22	0·300	23	0·072		2·832
25	1·572	27	0·210		6·025
28	0·030				
29	0·078		2·832		10·371
30	0·084				
	6·025				

VI. *Scientific Establishments in Ireland.*

It is with great satisfaction we notice the successful exertions which are made, even in the provincial cities of Ireland, to cultivate intellect, and advance and diffuse knowledge. For these purposes, there have been already created in Cork three public establishments: a chartered institution, with library, lecturers, apparatus, laboratory, museum, botanic garden, &c. &c.; also a public library, containing many thousand volumes in every department of literature, science, and the arts; but the establishment which indicates more than either of these the increasing intellectual excellence of that city, is "The Cork Philosophical and Literary Society," the members of which have frequent meetings for the purposes of reading and discussing the topics contained in essays written by some of their own body. The only subjects excluded are party-politics and religious controversy. It is evident that the Institution and the Library have sown the seed which is now bearing such luxuriant fruit in the Philosophical and Literary Society, the excellent papers of which, and the animated, interesting, and learned discussions they produce, will be alike beneficial and honourable to the citizens of Cork, and soon to the people of Ireland.

VII. *Volatility of Oxide of Lead.*

I am not certain whether chemists are aware of the great volatility of the protoxide of lead. I find that I can sublime it

and extending to the E. The height included an angle of nearly 40° from the earth. The columns moved regularly but slowly to the N.E. when the close of evening prevented further observation. On inquiry, the following morning, of some persons who lived in the direction of the cloud, I found it had been accompanied with heavy rain, a rushing sound like a waterfall, and a grayish mist, which, however, soon passed off.

in a common red heat. I had precipitated a carbonate of zinc, by dissolving common zinc in muriatic acid, and mixing the neutral solution with carbonate of soda. To convert this carbonate into oxide of zinc, I dried it in the open air, and then exposed it to a strong red heat in a platinum crucible covered with a lid. When the process was at an end, I found the inside of the lid coated with a substance resembling massicot. By digesting the lid in dilute nitric acid, I dissolved off the sublimate, and by evaporation obtained octahedral crystals of nitrate of lead. This unexpected fact induced me to visit a manufactory in Glasgow, where lead is reduced from galena, and afterwards converted into litharge. The volatilization of the lead in both processes was apparent; and I found on inquiry, that the workmen were quite aware of it. I have been told, that when lead is converted into litharge, and the litharge again reduced into lead by the common method, that the loss sustained rather exceeds 10 per cent.

#### VIII. *Queries respecting the Velocipede.*

(To Dr. Thomson.)

SIR,

As your journal embraces mechanics within the range of its notice, I am a little surprised that so curious and popular an invention as the Velocipede should have been hitherto overlooked. It is now about a year since I saw these whimsical machines in full action under the trees in the Jardin de Tivoli at Paris. I then anticipated the probability of their being converted to purposes of expedition; and accordingly on my return to England, a few months ago, I found them running everywhere throughout the kingdom. It is not because they are uncommon, therefore, that I now beg to call your attention to them; but it appears to me to be right that some account of the invention, with the improvements that have been made on it, should be registered in the contemporaneous numbers of a permanent and respectable journal. I hope, therefore, that you, Sir, or some of your mechanical correspondents, will favour us with an accurate description and plate of the machine, together with some remarks on its powers, and the improvements of which it is capable, as well as in the mode of working it. I regret to understand that it has frequently produced hernia in those who have exercised themselves with it. I should think this extremely probable; but it might be well that the fact were generally known.

I am, Sir, &c.

T. L. D.



ARTICLE XIII.

*Astronomical, Magnetical, and Meteorological Observations.*

By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*

Latitude 51° 37' 42" North. Longitude West in time 1' 20.7".

*Astronomical Observations.*

Aug. 10.	Emersion of Jupiter's second satellite .....	{ 11 <sup>h</sup> 38' 22"	Mean Time at Bushey.
		{ 11 39 43	Mean Time at Greenwich.
11.	Emersion of Jupiter's first satellite .....	{ 12 43 58	Mean Time at Bushey.
		{ 12 45 19	Mean Time at Greenwich.
27.	Emersion of Jupiter's first satellite .....	{ 11 02 40	Mean Time at Bushey.
		{ 11 04 01	Mean Time at Greenwich.

*Magnetical Observations, 1819. — Variation West.*

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
Aug. 1	8 <sup>h</sup> 40'	24° 32'	04"	1 <sup>h</sup> 40'	24° 41'	31"	7 <sup>h</sup> 30'	24° 34'	38"
2	8 40	24 31	31	1 15	24 42	04	— —	— —	—
3	8 40	24 32	44	1 30	24 45	30	7 25	24 34	48
4	8 40	24 34	01	1 20	24 44	34	7 25	24 34	39
5	8 45	24 33	53	1 20	24 43	38	7 25	24 33	15
6	8 40	24 34	10	1 20	24 42	20	7 20	24 34	16
7	8 35	24 32	42	1 25	24 42	36	— —	— —	—
8	8 40	24 31	27	1 35	24 42	27	7 25	24 34	54
9	8 35	24 31	46	1 25	24 44	15	7 25	24 34	50
10	8 45	24 32	07	1 30	24 44	16	7 30	24 34	47
11	8 35	24 32	14	1 40	24 42	26	— —	— —	—
12	8 45	24 32	40	1 20	24 43	36	7 25	24 35	58
13	8 35	24 34	03	1 10	24 42	33	— —	— —	—
14	8 35	24 32	36	1 30	24 42	24	— —	— —	—
15	8 35	24 32	55	1 35	24 41	57	7 25	24 35	27
16	8 35	24 33	42	1 10	24 41	03	— —	— —	—
17	8 30	24 31	36	— —	— —	—	— —	— —	—
18	8 35	24 32	08	1 20	24 41	18	7 20	24 33	05
19	8 45	24 33	52	1 20	24 42	42	7 15	24 33	19
20	8 40	24 33	09	1 25	24 42	05	7 20	24 34	23
21	8 35	24 32	44	1 25	24 42	40	7 15	24 34	15
22	8 40	24 33	31	1 20	24 41	45	7 15	24 34	16
23	8 35	24 31	15	1 25	24 42	53	7 15	24 34	25
24	8 40	24 30	02	1 20	24 42	58	7 10	24 31	49
25	8 40	24 41	37	1 25	24 45	57	7 10	24 31	50
26	8 30	24 34	07	1 20	24 42	27	7 10	24 33	44
27	8 40	24 33	27	1 25	24 42	18	7 10	24 33	40
28	8 30	24 40	13	1 15	24 45	18	— —	— —	—
29	8 40	24 30	09	1 55	24 41	35	7 05	24 34	52
30	8 40	24 31	28	— —	— —	—	7 05	24 33	42
31	8 30	24 31	54	1 20	24 40	42	— —	— —	—
Mean for Month.	{ 8 38	24 32 33		1 25	24 42 49		7 18	24 34 24	

In taking the mean of the observations in the morning, those on the 25th and 28th are rejected, being so much in excess, and or which there was no apparent cause.

## Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Aug		Inches.				Feet.		
1	Morn....	29.579	65°	59°	NE by N		Cloudy	60½
	Noon....	29.584	80	36	Var.		Fine	81½
	Even....	29.584	63		NE		Fine	57½
2	Morn....	29.543	60		NE		Cloudy	57½
	Noon....	29.533	67		NE		Cloudy	70
	Even....	—	—		—		—	55½
3	Morn....	29.467	60		NNW		Fine	69
	Noon....	29.467	67		NNW		Fine	58
	Even....	29.452	63		N		Cloudy	69
4	Morn....	29.437	60		N		Cloudy	59
	Noon....	29.453	65		NNW		Fine	60
	Even....	29.453	63		N by E		Drizzle	67
5	Morn....	29.479	60		N by W		Cloudy	59
	Noon....	29.506	64		N by W		Cloudy	59
	Even....	29.530	64		Calm		Cloudy	73
6	Morn....	29.560	62		SSW		Cloudy	60
	Noon....	29.553	71		SW		Showery	74½
	Even....	29.500	65		WSW		Fine	57
7	Morn....	29.558	65		WNW		Fine	71
	Noon....	29.558	73		W		Fine	59½
	Even....	—	—		—		Fine	78½
8	Morn....	29.629	61		NW		Very fine	59
	Noon....	29.652	70		NE by N		Very fine	58
	Even....	29.652	65		NW		Very fine	75½
9	Morn....	29.695	66		NE by N		Very fine	60
	Noon....	29.700	74		W by N		Cloudy	71½
	Even....	29.692	70		NNE		Showery	59
10	Morn....	29.665	66		ESE		Hazy	55
	Noon....	29.665	75		E		Fine	74
	Even....	29.648	64		E		Very fine	58
11	Morn....	29.560	63		ESE		Cloudy	75
	Noon....	29.539	73		SE		Cloudy	74
	Even....	—	—		—		Fine	58
12	Morn....	29.476	64		SE by S		Foggy	75½
	Noon....	29.490	72		Var.		Fine	60
	Even....	29.488	67		WSW		Cloudy	71½
13	Morn....	29.485	65		SW		Fine	57
	Noon....	29.490	68		SW		Sm. rain	75
	Even....	—	—		SW		Rain	61½
14	Morn....	29.590	62		N		Cloudy	74½
	Noon....	29.600	71		N		Cloudy	59
	Even....	—	—		—		—	78½
15	Morn....	29.658	64		N		Very fine	60
	Noon....	29.660	72		NW		Fine	73
	Even....	29.690	69		Calm		Cloudy	61½
16	Morn....	29.768	66		N		Fine	74½
	Noon....	29.778	74		NW		Fine	59
	Even....	—	—		—		—	78½
17	Morn....	29.820	66		NE		Very fine	60
	Noon....	—	—		—		Very fine	73
	Even....	—	—		—		Very fine	60
18	Morn....	29.872	63		NE		Cloudy	73
	Noon....	29.880	71		NE		Fine	73
	Even....	29.882	64		NE		Cloudy	73

*Meteorological Observations continued.*

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
Aug.	Morn....	29.603	62°		ENE		Sm. rain	56°
19	Noon....	29.785	70		NE		Cloudy	72
	Even....	29.757	66		NE		Fine	58
	Morn....	29.757	62		NE by E		Cloudy	70½
20	Noon....	29.765	68		NE		Cloudy	57
	Even....	29.765	65		NE		Cloudy	73½
	Morn....	29.783	61		NE		Cloudy	59
21	Noon....	29.772	71		NE		Fine	73½
	Even....	29.755	66		NE		Very fine	59
	Morn....	29.719	61		NE		Cloudy	73
22	Noon....	29.720	70		NE		Cloudy	59½
	Even....	29.692	65		NE		Clear	77½
	Morn....	29.690	64		NE by N		Very fine	57
23	Noon....	29.681	76		E		Very fine	77½
	Even....	29.669	68		E		Clear	57
	Morn....	29.633	65	65°	NNE		Clear	77½
24	Noon....	29.623	74	44	ENE		Very fine	58½
	Even....	29.582	69	49	E by S		Fine	77°
	Morn....	29.530	63	70	NNE		Fine	58½
25	Noon....	29.529	75	47	NE		Fine	77°
	Even....	29.557	67	55	NE		Fine	58½
	Morn....	29.612	63	70	NNE		Cloudy	77
26	Noon....	29.635	70	51	NE		Fine	57
	Even....	29.631	64	57	Calm		Cloudy	73
	Morn....	29.625	62	65	N by E		Fine	59
27	Noon....	29.624	71	53	NNE		Cloudy	72½
	Even....	29.600	65	60	NNE		Cloudy	57
	Morn....	29.508	62	70	ENE		Cloudy	70
28	Noon....	29.484	72	55	SE		Cloudy	56½
	Even....	—	—	—	—		—	67
	Morn....	29.248	62	70	SSW		Cloudy	50
29	Noon....	29.205	70	52	SW		Fine	61
	Even....	29.160	63	59	SSW		Very fine	56½
	Morn....	28.953	66	69	S		Showery	67
30	Noon....	28.810	—	78	SSW		Rain	50
	Even....	28.700	58	80	SSW		Showery	61
	Morn....	28.859	55	64	W		Fine	—
31	Noon....	28.888	61	51	W by S		Fine	—
	Even....	—	—	—	—		—	—

At three o'clock in the afternoon of the 3d, a violent thunder storm commenced; and in the space of rather more than an hour and a half, 1.81 inch of rain fell. Rain, by the pluviometer, between noon the 1st of August and noon the 1st of September, 2.52 inches. Evaporation during the same period, 4.72 inches.

## ERRATUM.

In the note to p. 185 of the last number of the *Annals*, for fore read foot.

## ARTICLE XIV.

## METEOROLOGICAL TABLE.

1819.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
		Max.	Min.	Max.	Min.			
8th Mo.								
Aug. 1	N E	30·05	30·04	86	53	—	16	56
2	N E	30·04	29·97	76	54	—		69
3	N W	29·97	29·96	71	57	—		58
4	N W	29·97	29·97	71	58	39		62
5	N W	30·02	29·97	69	53	—		68
6	S W	30·03	30·02	80	59	—	—	63
7	N W	30·11	30·03	81	50	32		63
8	N W	30·15	30·11	76	49	—		54
9	N W	30·15	30·13	81	50	—		57
10	S E	30·13	30·05	79	46	39		56
11	S E	30·05	29·99	80	49	—		62
12	S	29·99	29·97	83	60	—		68
13	W	30·08	29·99	78	58	—		58
14	N W	30·12	30·08	78	54	52		58
15	N	30·20	30·12	79	62	—		59
16	N	30·25	30·20	79	58	—		57
17	N	30·32	30·25	83	60	48		56
18	N	30·32	30·27	77	59	—		60
19	N	30·27	30·23	76	58	—		61
20	N E	30·25	30·23	75	55	—		58
21	N E	30·25	30·20	79	59	47		58
22	N E	30·20	30·16	78	54	—		57
23	N E	30·16	30·11	81	52	—		59
24	N W	30·11	30·02	84	50	49		56
25	N E	30·08	30·01	81	56	—		53
26	N E	30·08	30·08	74	53	—		54
27	N W	30·08	30·02	75	53	39		55
28	N W	30·02	29·80	80	51	—		57
29	W	29·80	29·38	78	54	—		57
30	S E	29·38	29·20	73	50	—	23	59
31	W	29·49	29·30	67	43	52	2	55
		30·32	29·20	86	43	3·97	0·41	53—69

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Eighth Month.*—1. Sultry weather: a heavy thunder storm at three, p.m. from the eastward. 2—5. Fine. 6. A few drops of rain in the afternoon. 7—25. Uninterrupted fine weather during this interval. 26—29. Chiefly cloudy weather. 30. Showery. 31. Showery: some hail in the afternoon.

## RESULTS.

Winds: N, 5; NE, 8; SE, 3; S, 1; SW, 1; W, 3; NW, 10.

## Barometer: Mean height

For the month. .... 30·031 inches.

For the lunar period (ending with the third quarter) 30·020

For 12 days, with the moon in south declination

(ending with the 8th inst.) . . . . . 30·055

For 15 days, with the moon in north declination

(ending with the 23d inst.) . . . . . 30·157

## Thermometer: Mean height

For the month. .... 65·88°

For 31 days with the sun in Leo. .... 66·60

## Hygrometer: Mean for the month (the extremes are now placed at

the foot of the column) . . . . . 59

Evaporation for the month. .... 3·97 inches.

Rain . . . . . 0·41

On the 1st of the month, after the thunder storm, a splendid meteor was seen by several persons at Tottenham, passing from the SE towards the W, letting fall sparks during its progress: the time appears to have been about a quarter before nine in the evening. The prevailing modifications of cloud during this month were the *Cumulus* and *Cirrus*. The *Cirrostratus* was almost banished from the sky, and the *Cumulostratus* appeared but little. The sudden depression of the barometer near the end was followed by 0·35 in. of rain at Tottenham: and although the gale which attended it was very moderate here, the effects were experienced in the north of England, and in Scotland, in a severe storm, denominated indeed by the reporter a *hurricane*, by which several vessels were driven on shore on the coasts of Cumberland and Scotland, and some of them totally lost. It would be acceptable to know, through the medium of this journal, the direction and changes of the wind, and the degree of depression observed in any good barometer situated near the level of the sea on this occasion.

Tottenham, Ninth Month, 18, 1819.

L. HOWARD.

# ANNALS

OF

## PHILOSOPHY.

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NOVEMBER, 1819.

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### ARTICLE I.

*On the Oxides and Salts of Mercury.* By Mr. Donovan.

(Concluded from p. 251.)

#### II. *Combination of the Sulphuric Acid with the Oxides of Mercury.*

39. I find that when strong sulphuric acid in any quantity is heated on mercury so gently that the metal barely effervesces, a white salt is speedily deposited, which is the real sulphate of mercury, containing no other than the black oxide. It dissolves very sparingly in sulphuric acid, whether hot or cold, and is decomposed by water.

40. When this sulphate is boiled in sulphuric acid, the acid is decomposed, the oxide takes up an additional portion of oxygen, and forms a white, crystalline, permanent salt, which contains no other than the red oxide, and is hence oxysulphate.

41. When sulphuric acid is violently boiled on any quantity of mercury, the metal is primarily brought to the state of black oxide, which may be detected in it; but the sulphate at this temperature, further decomposes the acid and forms oxysulphate. If the quantities be two of mercury to three of the strongest acid, the whole becomes oxysulphate; but if the ratio of acid be less, there will be an admixture of sulphate.

42. Berthollet forms the sulphate by boiling sulphuric acid diluted with its weight of water on mercury. In this case great caution is required; for the acid does not act until it becomes more concentrated by boiling; and during the progress of solu-



tion, the concentration may become so great as to form oxysulphate.

Thus it appears that the result of the action of sulphuric acid on mercury does not depend on the relative quantities of these bodies, but on the force of affinity produced by the degree of temperature.

43. When sulphate of mercury is decomposed by caustic alkaline solutions, a black powder is produced, which has been considered a subsulphate; but it will be shown presently that this is not the case. There is nevertheless a real subsulphate which has hitherto been overlooked, and of which a knowledge is essential to the right understanding of some of the salts.

If black oxide be triturated with sulphuric acid (1.090), its colour changes to gray, but it does not dissolve. If this powder welledulcerated be boiled in distilled water, and the liquor filtered, a solution of nitrate of barytes will occasion in it a copious precipitate. This is the real subsulphate of mercury. When formed in the cold, its colour is gray: when digested in boiling water, it loses still more of its acid, and becomes greenish-gray; but cold dilute sulphuric acid instantly restores its original colour by affording what it lost. Concentrated acid affords it still more, and renders it white. Thus the subsulphate varies in its composition.

44. This salt may also be formed by mixing solution of nitrate of mercury with sulphate of soda; a white precipitate appears which is subsulphate, near the point of saturation. Boiling water changes this to greenish-gray, as in the former case.

45. When mercury is dissolved by heat in its own weight of sulphuric acid, a saline mass is produced, which, when treated with boiling water, gives a fine yellow powder, often mistaken for Turbith mineral. It has this difference, however, that when decomposed by potash, a dark-brown powder is produced—a fact inexplicable without a knowledge of the existence of subsulphate of mercury. This brown powder, which might be mistaken for a distinct oxide, is a mere mixture of the black and red, arising from the subsulphate and suboxysulphate, which constituted the original yellow powder; and accordingly with muriatic acid, it forms calomel and corrosive sublimate. Turbith mineral, which has been long exposed to light, affords a brown precipitate upon the same principle.

46. Thus it appears that there are but two combinations of mercury with sulphuric acid, the sulphate and oxysulphate, with their subsalts; and that all the other varieties are but mixtures of these. To ascertain the composition of the oxides existing in these salts, I decomposed a quantity of sulphate of mercury (39) by means of pure potash; the black powder was well washed, dried, and freed from metallic mercury by trituration. This oxide, when submitted to analysis in the manner already described (13), afforded four per cent. of oxygen, which differs

from my former analysis of black oxide by only 0.04, and was probably owing to my not having expelled the whole of the water.

47. The oxysulphate (40), when precipitated by potash, afforded a yellow-coloured oxide, which agreed in its composition with that of red oxide prepared by calcination, except in containing 0.25 of water.

48. It has been conceived by some that these precipitates are not oxides, but subsalts. To ascertain how far this is true, I decomposed some subsulphate of mercury (44) by means of potash. The precipitate was repeatedly washed with distilled water, and was afterwards boiled almost to dryness with muriatic acid, which would separate any sulphuric acid that had adhered to the oxide. It was then boiled with a little distilled water, and filtered. In this liquor, muriate of barytes gave a scarcely perceptible cloudiness.

49. Some suboxysulphate was then decomposed by potash; the precipitate was repeatedly washed with distilled water, and dissolved in as much muriatic acid as was barely sufficient. In this solution, muriate of barytes gave no greater cloudiness than in the former case.

50. Hence, although these precipitates do retain a little of the acid, the quantity is so extremely minute that analysis scarcely detects it: and hence it is plain, that instead of subsalts, they should be considered oxides, as pure as the generality of substances obtained by art.

### III. *Combination of the Muriatic Acid with the Oxides of Mercury.\**

Chemists have hitherto been acquainted with but two combinations of muriatic acid and mercury; one with the black, and the other with the red oxide. I have found that the muriates fall within the general analogy of the mercurial salts, and that we can form the subsalts of those already known.

51. When calomel is 20 or 30 times boiled in different portions of distilled water, it will be found to have become gray. If the waters be evaporated down, they will afford indications of muriatic acid. This is, therefore, submuriate of mercury—a name which in the pharmacopœias has been erroneously attributed to the real muriate of mercury. It is also found when calomel is boiled in muriatic acid (1175); when black oxide of mercury is triturated in the cold with muriatic acid of the same specific gravity; or when calomel in a thin stratum is exposed to the rays of the sun. The best mode of obtaining it is by cold trituration, as above-mentioned.

52. Although strong muriatic acid takes acid from calomel, yet very dilute muriatic acid gives an additional dose to submu-

\* As to the new doctrine of chlorine, I here neither adopt nor reject it. I use the old terms, being more generally understood.

riate. Hence, by trituration of black oxide of mercury with very weak acid, we obtain a muriate very near the point of saturation; and by boiling very dark-coloured submuriate with dilute acid, the salt becomes nearly as white as calomel.

53. When this gray submuriate is exposed to heat in a subliming apparatus, a quantity of acid forsakes one part, and unites with the other: hence calomel sublimes, the oxide forsaken by the acid is reduced, oxygen being evolved, and a large quantity of mercury volatilized; but there is not a particle of oxymuriate formed. These changes exactly correspond with the constitution of a subsalt.

54. When solution of corrosive sublimate is boiled on red oxide of mercury, no change takes place at first, but after some time, the oxide suddenly becomes black.

This black powder, after being well washed, will afford the red oxide unaltered, when acted on by pure potash. If dilute nitric acid be poured on the black powder, it instantly dissolves, and the solution will be copiously precipitated by nitrate of silver. From these facts, and from the union of the red oxide with muriatic acid in a neutral solution of oxymuriate of mercury, the black substance is evidently a suboxymuriate.

This salt is scarcely soluble in water. However, if boiled in water, a little dissolves, which imparts a most disagreeable taste; the water, on cooling, deposits a small quantity of opaque crystalline grains, which are of a deep brown colour on account of the water which they contain.

55. It now remains to ascertain the constitution of the oxides which constitute the bases of the muriates of mercury. The decomposition of calomel has been already amply considered. The following is the theory of some other circumstances attendant on that process, which could not be explained without a previous knowledge of the suboxymuriate. When a few drops of solution of potash are let fall on calomel, a brown colour is given to it. The alkali takes up part of the acid, forming a submuriate, and reduces part of the oxide; the oxygen passing to the submuriate, forms a certain portion of brown suboxymuriate. When a sufficiency of alkali is poured on, the submuriate and suboxymuriate are decomposed, giving rise to black and red oxide. This theory accounts for the different changes of colour during the process, which are otherwise inexplicable.

56. The power which pure alkalies possess of partially reducing the oxide contained in the protosalts of mercury, might, perhaps, leave it doubtful what is the exact degree of oxidation of the metal in calomel. I conceive it to be the black oxide, as commonly supposed, on the following accounts. When nitrate of mercury is decomposed by acetate or muriate of soda, there is no reason to suppose any change in the oxidation of the metal: hence we conclude that calomel and acetate of mercury contain the same oxide. Now as the acetate is formed by

merely presenting distilled vinegar to the black oxide, we must infer this black oxide to be also the basis of calomel. Another reason is, that if a solution of black oxide in boiling distilled vinegar be mixed with common salt, calomel immediately forms. The analysis of this oxide has been already detailed.

57. The oxymuriate of mercury evidently contains the peroxide; for the fixed alkalies separate it unaltered, and the common red oxide, by simple solution in muriatic acid, affords oxymuriate. It appeared unnecessary, therefore, to ascertain its analysis by another trial.

58. If a solution of corrosive sublimate be exposed to the action of light, I have found that some calomel is formed, and, therefore, the oxide afforded by potash will not be the pure red oxide.

As to the oxides contained in the submuriate and suboxymuriate, there can be no doubt of their constitution.

59. The only remaining question on this part of the subject is, whether or not the precipitates obtained from the muriates of the two oxides be pure oxides or contain a portion of acid united to them. A solution of corrosive sublimate was precipitated with pure potash, the precipitate was well washed with distilled water, and dissolved in boiling dilute nitric acid very pure. To this solution was added solution of nitrate of silver; but the transparency was not in the least impaired, even when cold.

60. Calomel being decomposed by potash in the same manner, and its oxide dissolved in boiling dilute nitric acid, there was no change whatever produced by nitrate of silver. Hence these precipitates are pure oxides.

#### *IV. Combination of the Acetic Acid with the Oxides of Mercury.*

61. If distilled vinegar be boiled on black oxide of mercury, a simple solution takes place, and the acetate is deposited abundantly, as the fluid cools; or if distilled vinegar be boiled in nitrate of mercury, it is decomposed, and acetate forms in abundance.

62. If these crystals be separated, and triturated with a large quantity of water, they become yellow; and if the yellow crystals be boiled in water, they change to a blue grey powder. The same powder may be formed at once, by triturating black oxide with distilled vinegar. It is also slowly formed, when acetate of mercury is exposed to strong light. This salt, which has not hitherto been recognized, is subacetate of mercury.

63. When this salt is boiled in water, crystals of acetate are formed as the solution cools; for the subsalt is still further deprived of acid, and this, dissolving a portion of the subsalt, forms a quantity of the neutral salt. The yellow crystals above-mentioned are the subsalt, nearer to the point of saturation than the grey.



64. The oxacetate and suboxacetate are salts well known; and from the manner that these salts, as well as the two already mentioned, are formed, it is sufficiently evident that they contain no other oxides than the black and red.

*General Observations.*

Having now endeavoured to ascertain the composition of the two oxides of mercury, to enumerate the combination of the chief acids with the oxides, and to prove the identity of these oxides with those the composition of which had been previously discovered, it appears to me that there are no grounds for even supposing the existence of any other oxides than the black and red, or, what is the same thing, the yellow. In the progress of the inquiry, I have endeavoured to show, that in all the salts, the metal unites to the acid so as to produce a neutral and a subsalt with each oxide, the catalogue being completed by the subsulphate, submuriate, subacetate, and suboxymuriate. As to the supersalts of mercury, their existence has not been proved; in no case does the excess of acid enter into the salt as an essential ingredient constituting part of a definite compound, and retained by an affinity of any considerable force. And beside this, the analogy of the definite salts, as the muriate, the oxymuriate, and the acetate, manifest no tendency to unite with an excess of acid. Fourcroy found that the excess of acid was easily washed away from supersulphate of mercury; and the same might be applied to all the rest; for they may all be reduced to the state of subsalts by mere washing. The supersalts can only exist in solution, and they should rather be considered as solutions of the neutral salts in an excess of their own acid, which will again deposit these salts in a state of neutrality. Thus sulphate of soda crystallizes neutral in an excess of its own acid; but sulphate of potash in an excess of acid, will crystallize with this excess as often as we think proper, because that excess is retained by real affinity. The nitrate, sulphate, and acetate, will not dissolve in water of any temperature, unless there be an excess of acid: and if there be no such excess, one part of the salt supplies it, while a subsalt is deposited. This decomposition originates in the affinity of the water to acid, rather than of the salt to an excess of acid; for if the affinity of the water be previously satisfied by any other acid, the salt parts with very little of its own. Thus a solution of nitrate of mercury may be poured into a large quantity of water without decomposition, if the latter be acidulated with distilled vinegar, although there is no decomposition effected by the vinegar on the nitrate in the cold. If we attempt to saturate the excess of acid in a solution of nitrate of mercury, by an alkali, we fail; for every addition precipitates a subsalt, and yet a considerable excess of free acid is found in the solution to the last. It is, therefore, impossible to obtain a neutral mercurial salt while water is present; and it

is on this account that, by double decomposition, the resulting mercurial salt is always below the point of saturation (44).

*Appendix, containing an Account of a new Mercurial Ointment.*

In an attempt, already detailed (11), to obtain black oxide of mercury, I found that 60 gr. of metal, after 40 hours' trituration, afforded but 6 gr. of oxide. This fact, considered with the short time that is bestowed on the extinction of the mercury in the mercurial mills, excited my curiosity to examine the state of the mercury in that mercurial ointment which is used in the cure of the venereal disease, and of which the nature has been so much disputed.

I kept four ounces troy of mercurial ointment, prepared in a mill, about half a year before, at the temperature of  $212^{\circ}$ : it separated into two strata. When cold, the upper stratum was separated; it was of a light-grey colour. The under stratum was exposed to  $212^{\circ}$  on blotting paper, which soaked the remaining lard. The very heavy residue was triturated with a little magnesia, and it almost instantly afforded a quantity of mercury weighing 495 gr.; mere trituration with magnesia afforded 225 gr. additional. The earthy mass was put through a variety of processes which gave a quantity of globules, they may be rated at 60 gr. although I could not collect them all: a little oxide also appeared. Thus out of 960 gr. of mercury which the four ounces of ointment originally contained, there were recovered 770, which leaves 190 gr. the quantity of mercury apparently oxidized, or  $47\frac{1}{4}$  to each ounce. But the quantity is really much less.

The grey ointment which constituted the upper stratum appearing to contain oxide of mercury in a state of chemical combination, and believing that the lower stratum being metallic mercury could have no effect on the animal economy, when introduced into the circulation, I conceived that the medicinal effects of mercurial ointment must depend on the small quantity of oxide with which the fat is chemically combined. I, therefore, determined to try if this grey ointment would affect the human body in the same manner as the common mercurial ointment. I provided three females whose situation required the use of mercury. Each rubbed in a drachm every night. One was affected by the third rubbing, and the fourth put her under ptyalism. Another after rubbing three times was so salivated that she spit two quarts in the 24 hours; and this in a less degree was kept up for 10 days. The third was not affected until she rubbed six times, and then not considerably. They were all at length recovered.

Finding this ointment so very active, I conceived that by forming a chemical combination between fat and oxide of mercury in very small quantity, the same result might be obtained. I, therefore, kept lard and black oxide of mercury at



the temperature of about  $350^{\circ}$ , for two hours, continually stirring them. At the end of the process, it appeared that every ounce of lard had dissolved and united with 21 gr. of oxide.

I tried the effects of this ointment on several persons, and found it to be as active as the common mercurial which contains almost twelve times more mercury. One drachm could be rubbed in completely in from 6 to 10, or 15 minutes; while an equal quantity of common ointment will require 30 or 40 minutes. This to debilitated persons is a great advantage, the usual mode of friction being to such patients a most laborious undertaking. In but one or two cases, was any eruption produced on the part rubbed, and this was inconsiderable; but in the common mode, the pustules are often so painful and numerous as to preclude the further continuance of friction. The use of the new ointment is extremely cleanly, the parts being left scarcely discoloured; and this, when privacy is an object, is of great importance. The last advantage, and to the hospitals not an inconsiderable one, is the comparative cheapness of the new ointment: the common mercurial ointment costs in Dublin from 4s. to 5s. a pound; the other can be prepared at an expense little above that of the lard.

For the preparation of this ointment, it is essential that the lard be entirely free from salt; if not, calomel will be formed.

The oxide may be prepared by decomposing calomel with solution of pure potash; or, which is cheaper, by pouring solution of nitrate of mercury into caustic alkaline solution.

Although the fat will scarcely dissolve more than 3 gr. of oxide to each drachm, the quantity of oxide may be more if necessary. The ointment that I employed was merely saturated. The oxide should be first triturated with a little lard in the cold, to make the penetration complete.

The degree of heat at which the combination is attempted is material. At  $212^{\circ}$  the oxide and lard will not combine; at  $600^{\circ}$  the oxide will be decomposed, and mercury volatilized; at  $500^{\circ}$  and  $400^{\circ}$ , the oxide is partially decomposed, some red oxide being formed, and mercury reduced. The best heat is between  $300^{\circ}$  and  $320^{\circ}$ ; it should be maintained at least an hour, and the ointment should be stirred till cold.

An ointment composed of what has been called ash-coloured oxide of mercury mixed with lard was formerly in use; but was found not to answer the purpose. The reason is plain. The oxide contained a mercurial salt, not soluble in fat; and the oxide was not combined, but mixed in the ointment.

Whether the cure effected by this ointment be permanent, experience alone can decide. I see no reason for the contrary opinion. It contains as much oxidized mercury, in a state of combination, as the common ointment; the metallic mercury in the latter being not only useless, but hurtful, by obstructing the absorbents, and thus retarding the entrance of the oxide into the circulation, and by exciting a diseased action in the surface rubbed,

This ointment has been used by many of the most eminent surgeons in Dublin, both in private and hospital practice. It is at present under trial in some of the public institutions ; but the results, though favourable, I am not at yet at liberty to publish. In the Lock Hospital it has been extensively used, and is still under use. Such of the surgeons of that institution as have already tried it extensively enough to form a decided opinion, have favoured me with communications on the subject : these I think it necessary to make known.

*Mr. Johnston's Letter.*

" MY DEAR SIR,

Temple, Aug. 8, 1817.

" I have complied with your desire of administering your new mercurial ointment for the purpose of ascertaining its advantages as a remedy in venereal affections ; and it is with much satisfaction I have to state that the result of several trials has fully confirmed your expectations.

" Eight patients were selected from those under my care in the Westmoreland Lock Hospital, who had not used any mercury, and were put on a course of rubbings of this ointment. Every one of them was able to rub in a drachm of it, perfectly dry, in the short space of 15 minutes, and the succeeding rubbing seldom required more than 12 or 13 minutes. Even those in a weak state of health, from previous indisposition, experienced equal facility with the most robust, in effecting this operation. It produced the characteristic effects of mercury on the system in general in a shorter period than I have observed to arise from the employment of an equal quantity of the common blue ointment.\* In no case did the frictions with your ointment produce any cutaneous eruption of the parts to interrupt the continuance of its use, an effect which so often arises from the use of the old ointment, and which obliges the practitioner to change this mode of exhibiting mercury, perhaps greatly to the injury of his patient.

" These are advantages which, in my opinion, give the new ointment a decided preference amongst the profession as an anti-venereal remedy, and render it a valuable addition to the articles of *the materia medica*. I regret that circumstances have obliged me to be so brief, but for particulars I refer you to the inclosed cases.

" With every sentiment of regard,

" I am, my dear Sir, yours, most sincerely,

" To M. Donovan, &c."

" ANDREW JOHNSTON."

\* This fact I also observed ; and it has been remarked to me by surgeons, that the new ointment arrests the progress of the disease more speedily than the common, owing no doubt to the facility and completeness of its absorption.—M. D.



*Mr. Roney's Letter.*

" MY DEAR SIR,

47, *Great Dominick-street*, Sept. 16, 1817.

" In reply to your note of the 10th inst. requesting my opinion of the ointment which I have been for some weeks past using in my wards of the Westmoreland Lock Hospital, and also in private practice, I beg to say in direct answer to the three queries you have put to me,

" First, As to its facility in being rubbed in.

" Answer.—I have in almost all the cases found one drachm to be rubbed in in the space of 10 minutes, sometimes in 5, and never exceeding 15 ; in general eight minutes would be the fair average.

" Secondly, As to its cleanliness.

" Answer.—It leaves little, if any, appearance after its use. In private practice, this is of great importance. In two particular and very delicate cases under my care the ointment has in this respect given the greatest satisfaction, it not having left any appearance on the patients' linen, &c.

" Thirdly, Does it bring out an eruption.

" Answer.—In no case has it brought out any, and I have put an entire ward in the Lock Hospital under its use.

" The above are the answers to your queries. From my public situations I have had opportunities of trying this valuable improvement in almost every stage of the venereal disease. The symptoms were in all cases relieved by it ; many discharged as cured ; and the others are in a state of progressive improvement. I feel it my duty to say that sufficient time has not yet elapsed to ascertain the permanency of the cure ; but if it would not be considered premature, I would give as my opinion, that this ointment is as well calculated to remove the complaint as any other preparation of mercury, and certainly possessing the above-mentioned advantages, which in my mind are calculated to recommend its most general use, and to make both the public and profession feel much indebted to you for its introduction.

" I remain, my dear Sir, with much esteem,

" Yours, very truly,

" To M. Donovan, &c."

" CUSACK RONEY."

*The Surgeon-General's Letter.*

" SIR,

*Merrion-square*, Sept. 1, 1817.

" I have the honour to transmit herewith an extract of a report from Staff-Officer Assistant Shield, containing the result of the trials which have been made in the King's Military Infirmary of the mercurial ointment procured from you.

" I have the honour to be, Sir, your obedient humble servant,

" To M. Donovan, &c."

" PHILIP CRAMPTON."

"The new mercurial ointment has been tried in two cases of venereal ulceration of the tonsils, and one of chancre with bubo. In all, the symptoms yielded regularly, and the cure was effected in the usual time. In one case, the mouth became sore on the fifth day; in the second, on the 16th day; in the third, there was no sensible affection of the mouth. In none of the cases was there any disturbance of the bowels. The advantages of the ointment, as observed in the three cases alluded to, were, that one drachm of it could be rubbed in effectually in about 12 minutes; that it brought out no pustular eruption on the parts to which it was applied, and that it left no stain upon the skin, or upon the bed-clothes. (Signed) J. SHIELD,

"King's Military Infirmary, Phoenix Park."

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## ARTICLE II.

*Reply to Mr. Meikle on Centrifugal Force.*

By the Rev. Patrick Keith.

(To Dr. Thomson.)

SIR,

*Bethersden Vicarage, Kent, Sept. 13, 1819.*

YOUR correspondent Mr. Meikle, who has favoured us with an article on centrifugal force, and the upright growth of vegetables, in your number for July last, condescends to make some brief remarks on my paper on Mr. Knight's hypothesis, in your number for April preceding. I am represented as entertaining singular ideas of a centrifugal force produced by rapid circular motion. What I have said is, that "the direction of the centrifugal force in question must of necessity have been oblique, as being the simple effect of circular motion." Perhaps I should have said a rotatory motion instead of a circular motion; and, perhaps, the term oblique is not sufficiently applicable. But it is plain that I am speaking merely of the centrifugal force, or acquired momentum, by which the wheel was now capable of throwing off a body from its circumference in the direction of a tangent, as will appear from my referring immediately after to the possibility of a bean's being thrown off from the rim; and will Mr. Meikle say that this was not a consequence of its rotatory motion? It was entirely so; and was a capability that no other motion could have communicated to it. For though the wheel (if otherwise moveable) had been pulled backwards, or forwards, or upwards, as quick as you please, still it would have thrown off no body that might have been lodged on its surface in the direction of a tangent. Nor are my ideas of this force so singular as Mr. Meikle has imagined. For Mr. Knight represents the centrifugal force of his wheel as being always in proportion to the velocity, and,



consequently, as being an effect of the rotatory motion. The same conclusion may be deduced from premises furnished even by Mr. Meikle. He maintains that the centrifugal force acts directly from the centre. Be it so; and let me ask where was the force that acted directly from the centre before the commencement of the rotatory motion?

After all, I am not very sure that I have caught Mr. Meikle's meaning on this point; and it may be that I have been combating a phantom. If so, my only fault is, my "seeming to hold out that the centrifugal force acts in the direction of a tangent to the orbit, and not directly from the centre." This is represented as a notion absurdly and peculiarly my own, and treated with an attempt at merriment, as well as with an air of very profound knowledge. But I think I may fairly return the compliment, and contend that Mr. Meikle entertains notions on the same subject which seem to be peculiarly *his* own.

His first peculiarity is his establishment of what he calls the well-known fact, "that the centrifugal force acts directly from the centre," and not in the direction of a tangent to the orbit which the moving body is describing. This he regards as being established by the simple experiment of whirling a sling around the head. But unless Mr. Meikle attaches some peculiar meaning to the term *centrifugal force* of which I am not aware, I cannot see how this experiment is any proof of the doctrine which he maintains. Is it not true that a circular motion is produced by the joint action of two forces; one, that would impel the body in a tangent to the orbit, if left to itself, called a centrifugal force; and another, that would attract it to the centre, if left to itself, called a centripetal force? And is not this doctrine confirmed from the phenomena of projectiles? What will Mr. Meikle say of the centrifugal force of a cannon-ball that is discharged from the mouth of a cannon, placed either horizontally, or at any angle short of  $90^\circ$ . At all the different elevations of which it is thus capable, it has acquired a centrifugal force, and yet that force is in no one case directly from the centre of the earth. Does Mr. Meikle refuse to call the projectile impulse a centrifugal force, or is there any other centrifugal force in the case?

But to return to Mr. Meikle's own example. In consequence of the intervention of the sling that connects the stone and hand, and makes the latter seek the former as much as the former seeks the latter (as it is said), the centrifugal force seems no doubt to act directly from the centre, because the hand that twirls the sling is placed in it; though it is evident that the circular motion was communicated to the stone only by the original swing that was given to the sling in the direction of the first swing of a pendulum, and so carried round by the living power of the hand, which, during the experiment, must have been continually in action. This swing was the primary centri-

fugal force, and the cohesion of the parts of the string was the centripetal force. But if there is another centrifugal force that acts directly from the centre, then it is either a force that has been generated without any apparent cause, or it is a force that could in no way contribute to circular motion.

If this is doubted, let any one fix a stone to the one end of a string, and try to give it the rotatory motion by throwing it straight upward, or forward, from his hand holding the other end of the string (which is a centrifugal force acting directly from the centre), and he will find that the stone, instead of revolving in a circle, will either drop perpendicularly downwards, or descend like the ball of a pendulum, as soon as it reaches the end of the string. The motion, therefore, which is communicated to a body that is impelled by a force acting directly from the centre can be only rectilinear, like that of an arrow shot from a bow perpendicularly upwards, and not curvilinear, like that of a sling twirled round the head; and the force which contributes to circular motion can never be directly from the centre. Thus, no force acting directly from the centre could have communicated to Mr. Knight's wheel a motion round its own axis—a mere *axis in peritrochio* that can be put in motion only by a power applied in the direction of a tangent to the circumference, which power the water was that fell into the cogs of the main wheel, and put the whole machinery into motion.

Mr. Meikle's next peculiarity is his maintaining that "when a body is relieved from moving in a circle, its flying off in a tangent is in perfect harmony with, nay a consequence of, the centrifugal force's acting directly from the centre." It would certainly be edifying to see the proofs of the doctrine which Mr. Meikle thus advances; that is, to know how a change of motion may be effected in a line of direction different from that of the force impressed. But till the alleged proofs may happen to fall in my way, I shall consider myself as at liberty to suppose, that when a body is relieved from moving in a circle, its flying off in a tangent is a consequence of the centrifugal force's acting in a line that is not directly from the centre; for where a force acts directly from the centre, it can have nothing to do with circular motion. Nor is the flying off even in a tangent to be regarded as an invariable and necessary consequence of relieving a body from moving in a circle. The consequence may be very different. For a body may be relieved from moving in a circle by the destruction of the centrifugal force itself, and then it will not fly off in a tangent, but will assume a direction depending upon the direction or position of the cause that obstructs its progress. Suppose the motion of a twirled sling to be obstructed by its striking against some obstacle, instead of going completely round, and you have an example in point. But if the body is relieved from moving in a circle by the destruction of the centri-



petal force only, it will then fly off in a tangent, as the experiment of a stone fairly thrown from a sling will show.

I have said that the radicles ought to have been elongated in the direction of a tangent; but the relief wanted had partially taken place, at least upon Mr. Knight's principles. For Mr. Knight says, that in the case of the vertical wheel performing 150 revolutions in a minute the influence of gravitation was conceived to be wholly suspended, and consequently the centripetal force. The radicles were, therefore, subjected to the agency of the centrifugal force alone; and though they would still have been compelled to revolve in a circle, from their indissoluble connexion with the wheel; yet as Mr. Knight regards the matter which is added to the apex or point of the radicle as "being fluid, or changing from a fluid to a solid state," and hence sufficiently susceptible to the influence of gravitation to give it an inclination downwards, when lying at rest in the earth, I thought I might assume the same fluidity in the case of the beans attached to the wheel, and contend that similar causes should produce similar effects; or, that the continual tendency of the beans to fly off in a tangent ought to have made the fluid particles that were successively added to the points of their germinating radicles to assume the direction of a tangent also. But this notion I have, perhaps, carried rather too far.

There is a curious consequence that would result from Mr. Knight's doctrine, if true, which has just occurred to me. If gravitation is the true cause by which the roots of plants are made to descend, and their stems to ascend; and if the result of Mr. Knight's experiments is to be regarded as a proof of the fact; then, if the diurnal or rotatory motion of the earth were to be but sufficiently accelerated, the roots of trees would grow upwards, and the stems downwards, the earth acting the part of the upright and revolving wheel.

*Spectatum admissi risum teneatis amici?—*

I have but little more to add in reply to Mr. Meikle's remarks, in which I cannot but admit that he has given us a very conspicuous display of his knowledge of the principles of mechanics, and acquaintance with the laws of motion, and composition of forces; but unfortunately he has done nothing to clear up the obscurity of the point in question, or to help us out of our difficulties on either side. In short, he has left us precisely where we were before. For allowing that his strictures on my paper were even well founded, still they affect but a mere flaw—a mere trifle—a mere speck in the colouring, that tends nothing to the prejudice of the main arguments which I adduce. The merits of my paper can be estimated only by a minute examination of the phytological facts which it exhibits, and of the legitimacy of the conclusion which I have thought myself entitled to

draw from them; and this I should have been very glad if Mr. Meikle had been pleased to undertake. I am, Sir,

Your most obedient humble servant,

P. KEITH.

### ARTICLE III.

*Experiments on the Gas from Coal, chiefly with a View to its Practical Application.* By William Henry, M.D. F.R.S. &c.\*

THE chemical properties and composition of the gas from coal formed a principal object of two different series of experiments, the results of which I laid before the public many years ago. The first of these communications, entitled "Experiments on the Gases obtained by the destructive Distillation of Wood, Peat, Pit-Coal, Oil, Wax, &c. with a View to the Theory of their Combustion, when employed as Sources of Artificial Light," appeared in Mr. Nicholson's Philosophical Journal for June, 1805;† and the second memoir was published in the Transactions of the Royal Society for 1808.

By the first train of experiments, I endeavoured to derive, from a careful analysis of the compound combustible gases, a measure of their illuminating power, admitting of more exact appreciation than the optical method of a comparison of shadows. The one which I was led to propose as the most accurate, and which I still think entitled to preference, was the determination of the quantities of oxygen gas consumed, and of carbonic acid formed, by the combustion of equal measures of the different inflammable gases; that gas having the greatest illuminating power, which, in a given volume, condenses the largest quantity of oxygen. The average results of a great variety of experiments were comprised in the following table:

Kinds of gas.	Oxygen gas required to saturate 100 measures.	Carbonic acid produced.
Pure hydrogen .....	50 .....	—
Gas from moist charcoal .....	60 .....	33
wood (oak) .....	54 .....	35
dried peat .....	68 .....	43
cannel coal. ....	170 .....	100
lamp-oil .....	190 .....	124
wax. ....	220 .....	137
Olefiant gas. ....	284 .....	179

\* From the Memoirs of the Literary and Philosophical Society of Manchester vol. iii. Second Series.

† 8vo. Series, vol. xi. p. 65.

In the same essay, I maintained an opinion, which, on the most mature consideration, I see no reason to change; that the great variety of gases evolved by the destructive distillation of inflammable substances, do not constitute so many distinct species, but are mixtures of a few, the nature and properties of which were before ascertained. It will contribute to render what follows more intelligible, if a brief account be given of those gases of known composition, the mixtures of which, in various proportions, compose, according to this view, all the observed varieties; and I shall make their comparison under a form best adapted to illustrate their practical application.

1. *Hydrogen Gas*.—This is the lightest of all known gases; its specific gravity, that of atmospheric air being taken at 1000, being about 73. As ordinarily procured, by the solution of iron or zinc in diluted sulphuric acid, it contains impurities which give it a disagreeable smell; but well purified hydrogen has little if any odour. It burns with a pale and feeble flame, not at all suited to artificial illumination.

Product of its combustion.

	Grains.	Grains.
The cubic foot weighs about .....	40	
Consumes half a cubic foot of oxygen ..	300	
	<hr/> 340	Water .... 340

2. *Carburetted Hydrogen* has been shown to constitute the gas of marshes, and the fire-damp of coal mines. In these natural forms, it is contaminated with a small proportion of carbonic acid, and a larger one of azotic gas, but appears to be free from all other impurities. It is proved to be a definite compound of hydrogen and charcoal without any oxygen. It is lighter than common air, in the proportion of about 600 to 1000; it has very little odour; and burns with a flame greatly surpassing that of hydrogen, in density, and illuminating power.

Products.

	oz. dr.*		oz. dr.
A cubic foot weighs. ..	0 : 12	1 cubic foot of carb. acid	1 : 13
Consumes 2 cubic feet of			
oxygen .....	2 : 10	Water. ....	1 : 09
	<hr/> 3 : 06		<hr/> 3 : 06

3. *Carbonic Oxide* is rather lighter than common air. It contains no hydrogen, and is purely a compound of charcoal and oxygen, the latter being in just half the proportion which is required to constitute carbonic acid. It burns with a feeble blue light.

\* The avoirdupois ounce of 437½ gr. or 16 drams, is to be understood.

	oz. dr.	Product.	oz. dr.
A cubic foot weighs ...	1 : 03		
Consumes half a cubic foot of oxygen .....	0 : 11		
	<hr/> 1 : 14	Carbonic acid .....	1 : 14

4. *Olefiant Gas, or Bicarbuiretted Hydrogen.*—This has been demonstrated to be a compound of nearly 85 by weight charcoal, and 15 hydrogen, without any oxygen. It is a little lighter than common air, viz. in the proportion of about 974 to 1000. It surpasses all other gases in the brightness and density of its flame. Its name was originally derived from the property, which it possesses, of being speedily and entirely condensed, by rather more than an equal volume of chlorine gas, into a liquid resembling oil in appearance, but since shown to approach more nearly to the nature of ether.

	oz. dr.	Products.	oz. dr.
A cubic foot weighs ...	1 : 3	2 cubic feet carb. acid. .	3 : 10
Consumes 3 cub. feet of oxygen .....	4 : 0	Water .....	1 : 09
	<hr/> 5 : 3		<hr/> 5 : 03

Olefiant gas I found to be one of the products of the distillation of oil and of bees' wax, and was led, therefore, to suggest, that the wick of a lamp or candle, surrounded by flame, is to be considered as a bundle of ignited capillary tubes, into which the melted inflammable matter is drawn, and there resolved, not into a condensable vapour, but into olefiant and carburetted hydrogen gases. In the gas from coal, also, I detected the presence of olefiant gas, by the test of the action of chlorine.

In the second series of experiments,\* I submitted to distillation, on a small scale, various kinds of coal, from different parts of the kingdom. The æriform products, at different stages of the process, were kept apart, and were separately analyzed. From coal distilled in small iron tubes or retorts, which, when filled, were placed at once in a low red heat, small quantities of sulphuretted hydrogen and carbonic acid gases came over at first, in mixture with the other gases, but in a gradually diminishing proportion, till at length, in the last products, they were not discoverable at all. The production of olefiant gas observed the same order, and a gradual diminution took place, as the process advanced, in the combustibility of the gas, as determined by its requiring less and less oxygen for saturation. A great variety was ascertained to exist in the quality of the gas from different

\* Phil. Trans. 1808, p. 282.



kinds of coal ; that from Wigan cannel, holding the highest rank in illuminating power, and that from the stone coal of South Wales, the lowest.

Since the period when the second of these papers was published, the use of artificial gases, as a source of light, has been rapidly increasing in this, and, I believe, in other countries, and promises to attain an extent and importance sufficient to justify any labour that may tend, however remotely, to its improved application. It has frequently happened of late years that I have been requested by the proprietors of large manufactories lighted by gas in this neighbourhood, to give an opinion on practical points, respecting some of which I felt myself incompetent to decide, from the want of the necessary data. It is to supply these data that I have once more returned to the investigation of the subject. The objects, which I have had it in view to determine by the following course of experiments, are, whether, on the large scale of manufacture, there is a decline in the value of the æriform products of coal, from the beginning to the end of the distillation, similar to that which takes place on a small scale ; at what stages of the process those gases, which may be considered as impurities, are chiefly evolved ; and whether they are essential or accidental products ; whether the method of removing the sulphuretted hydrogen and carbonic acid gases by quicklime, which I suggested in the second memoir, is adequate to the complete purification of coal gas ; whether this purification is attended with any loss of that portion of the gas, which on account of its superior illuminating power, it is desirable not to remove ; and, if such a loss should be found to ensue, whether it may not be avoided by some modification of the purifying process. In determining these points, I was indebted for the necessary supplies of gas to Mr. Lee, at whose extensive manufactory the principal facts were ascertained that formed the basis of the first accurate calculations respecting the economy of gas from coal.\*

*On the Quality of the Gas at different Stages of the Distillation.*

The gas which I first submitted to experiment was obtained from Wigan cannel coal, a substance preferred in this neighbourhood as affording æriform products, which, both by their quantity and quality, more than compensate its higher price.† The retorts are charged while red-hot with this substance, and indeed are never suffered, during the whole of the winter season, to fall below the temperature of ignition. The gas was collected in a bladder furnished with a stop-cock, which was fixed into an opening in the pipe between the retort and the tar-pit. It was taken at this place, in order to avoid contact with water, and

\* See Mr. Murdoch's "Account of the Application of the Gas from Coal to economical Purposes." *Phil. Trans.* 1808, p. 124.

† About a shilling per cwt. of 112lb. or 13½d. delivered in Manchester.

admixture with any atmospherical air, that might accidentally remain in the gasometer. Wishing to examine the gas in a perfectly recent case, and finding it impossible to make the necessary experiments with sufficient accuracy in a shorter interval, I was obliged to be satisfied with procuring it every other hour. In this place, I shall only state the general results, and I shall describe, in a subsequent part of the paper, the methods of analysis, in order that other persons who may choose to compare my experiments with their own may conduct them under equal circumstances.

By the expression *impure gas* is to be understood the gas precisely in the state in which it was collected from the retort; and by *purified gas*, the same product after being freed from carbonic acid and sulphuretted hydrogen by solution of pure potash, applied in very small quantity, relatively to the volume of the gas, and with the least agitation adequate to the effect.

TABLE I.

*Showing the Quality of Gas from 1120lb. of Cannel, at different Periods of the Distillation.*

Hours from the commencement.	100 measures of impure gas contain of		100 measures of purified gas consist of			100 measures of purified gas	
	sul. hyd.	carb. acid	olef.	other infl. gases.	az.	consume oxygen.	give carb. acid.
$\frac{1}{2}$ an hour ..	$0\frac{1}{2}$	$5\frac{1}{2}$	16	64	20	180	94
1 hour. ....	3	$3\frac{1}{2}$	18	$77\frac{1}{2}$	$4\frac{1}{2}$	210	112
3 hours ....	$2\frac{1}{2}$	$2\frac{1}{2}$	15	80	5	200	108
5 hours ....	$2\frac{1}{2}$	$2\frac{1}{2}$	13	72	15	176	94
7 hours ....	2	$2\frac{1}{2}$	9	76	15	170	83
9 hours ....	$0\frac{1}{2}$	$2\frac{1}{2}$	8	77	15	150	73
$10\frac{1}{2}$ hours ....	0	2	6	74	20	120	54
12 hours ....	0	$0\frac{1}{2}$	4	76	20	82	36

Excluding from the calculation the azotic gas, with various proportions of which the products were contaminated, the following table shows the quantity of oxygen gas consumed, and of carbonic acid produced, by the really combustible part of the gas.

TABLE II.

*Showing the Quality of the really combustible Part of the Gas at different Periods of Distillation.*

	Take oxygen.	Give carb. acid.
100 measures of half hour's gas.....	225	118
1 hour's gas.....	220	117
3 .....	210	114
5 .....	206	108
7 .....	200	98
9 .....	176	85
$10\frac{1}{2}$ .....	150	70
12 .....	103	45



The next set of experiments was made on gas from common coal, got at Clifton, near Manchester, and of fair average quality.

TABLE III.

*Showing the Quality of the Gas from 1120lb. of Common Coal at different Periods of the Distillation.*

	100 measures of im- pure gas contain		100 measures of purified gas contain			100 measures of pu- rified gas	
	sul. hyd.	carb. acid	olef.	other infl. gases.	az.	consume oxygen.	give car. acid.
1 hour's gas..	3	3	10	90	0	164	91
3 hours' gas..	2	2	9	91	0	168	93
5 hours' gas..	3	2	6	94	0	132	70
7 hours' gas..	1	3	5	80	15	120	64
9 hours' gas..	1	2½	2	89	9	112	60
11 hours' gas..	1	1	0	85	15	90	43

Exclusive of the azote, with which the three last portions o. gas were mingled, they consumed oxygen, and gave carbonic acid as follows. The seven hours' gas in this instance, as sometimes happens from irregularities of temperature, was more combustible than that collected two hours sooner.

	Consumed oxygen.	Gave carb. acid.
100 m. of 7 hours' gas .....	140	75
9 .....	123	66
11 .....	106	50

A comparison of the results exhibited in the third table with those of the distillation of cannel coal is greatly in favour of the latter substance as a source of light. This will appear most distinctly by setting against each other the proportions of oxygen which are consumed by the gases evolved from the two substances at equal times from the commencement.

TABLE IV.

*Comparative Table of the Qualities of the Gases from Wigan Cannel, and from common Coal, at equal Times from the Commencement of the Distillation.*

	Oxygen consumed by 100 m. of cannel gas.	Oxygen consumed by 100 m. of Clifton coal gas.
1 hour's gas. ....	220	164
3 .....	210	168
5 .....	206	132
7 .....	200	140
9 .....	176	123
11 .....	150	106

It appears from these experiments, that the gas from cannel

has, in an equal volume, an illuminating power about one-third greater than that from coal of medium quality. The quantity also from the former substance exceeded by about one-seventh that obtained from coal distilled under precisely similar circumstances; 3500 cubic feet of gas having been collected from 1120 pounds of cannel, and only 3000 cubic feet from the same quantity of coal. The whole product of one distillation of cannel mixed together in a gasometer was of such quality that 100 measures required for combustion 155 measures of oxygen gas, and gave 88 measures of carbonic acid. But as the gas was contaminated with 15 measures of azote in every 100, the oxygen required for saturating 100 measures of the really combustible part of it may be stated at 195; and the carbonic acid produced at 110. It may be necessary to observe, that in comparing the value of gases produced from different kinds of coal, or from the same kind of coal differently treated, it is not enough to determine the *quantity* of aëriform products; and no satisfactory conclusion can be drawn respecting the relative fitness of any variety of coal for affording gas, or the advantages of different modes of distillation, unless the *degrees of combustibility* of the gases compared be determined, by finding experimentally the proportion of oxygen gas required for their saturation.

The results expressed in the first table, when contrasted with those which I formerly obtained by the destructive distillation of small quantities of coal, present several circumstances of disagreement, as to the quality of the products at different stages of the operation. In small experiments, the sulphuretted hydrogen and carbonic acid gases were evolved only at the early stages of the process; and sulphuretted hydrogen especially could not by the nicest tests be discovered in the last products of gas. On the large scale both these gases continue to be evolved throughout the whole operation, though in greatly diminished proportion towards the latter end. Even in the advanced stages of large distillations, the presence of sulphuretted hydrogen in coal gas may be traced by the proper test, though not in a quantity that admits of being easily measured. The test, which I used for some time, was the white oxide of bismuth, for which I afterwards substituted white lead, ground with a little water to the proper consistence, and spread by a camel's hair pencil on a slip of card. This was secured by a small pair of forceps fixed in a cork, by means of which the slip of card could be placed in a jar or bottle of the gas, and kept there for some time. By experiments on artificial mixtures, I found that a cubic inch of sulphuretted hydrogen diffused through 20,000 cubic inches of common air distinctly affected the test, which it changed to a light-yellowish or straw colour. By mixing sulphuretted hydrogen with various proportions of common air, I prepared coloured cards of a variety of shades, which served as standards of comparison for judging of proportions of sulphuretted hydrogen in coal gas, which were too minute to be accurately measured.

In the small experiments made several years ago, I never found, in the early products of gas from cannel coal, a proportion of olefiant gas at all approaching that which is noted in Table I, and its quantity in small distillations rapidly decreased, until in the latter products it could be no longer traced at all. The method of analysis, which I formerly employed, led me, however, as I have lately discovered, to under-rate the proportion of olefiant gas, and to over-estimate that of sulphuretted hydrogen. But making due allowance for this error, the superiority of the products of large operations, so far as respects olefiant gas, still exists, and is confirmed by comparative experiments on a small scale which I have lately made. Thus it appears from Table I, that even after 12 hours continuance of the process, olefiant gas still constitutes four per cent. of the gases evolved from cannel. The other inflammable gases also, when obtained in large quantity, are more uniform in quality, and possess, towards the close of the process, much greater combustibility and illuminating power than when procured in small experiments. This superiority is obviously dependent on the greater facility of preserving an uniform temperature in all chemical processes which are carried on upon a scale of magnitude.

The temperature to which the coal is subjected must necessarily be a point of the greatest importance to the quantity and quality of the æriform products; for while too low a heat distils over in the form of a condensible fluid, the bituminous part of the coal which ought to afford gas, too high a temperature, on the contrary, occasions the production of a large relative proportion of the lighter and less combustible gases. It would be a great step in the improvement of the manufacture of coal gas if the whole of the hydrogen could be obtained in combination with that proportion of charcoal which constitutes olefiant gas; and it is satisfactory to know that no impediment to this arises out of the proportion of the hydrogen and charcoal present in coal. If this object be ever accomplished, it will probably be by the discovery of means of uniformly supporting such a temperature as shall be adequate to the production of olefiant gas, and shall never rise above it; and some probability of success is perhaps derivable from the fact, that M. Berthollet, by the careful decomposition of oil, which in my experiments afforded a mixture of gases, succeeded in obtaining olefiant gas in a state of purity.\*

With the view of ascertaining how low a degree of heat is adequate to the production of gas from coal, I placed a small iron retort containing cannel in melted solders of various composition without obtaining more than the common air of the vessel. The retort, charged with fresh materials, was then immersed in melted lead; but after expelling the common air,

\* *Memoires de la Soc. d'Arcueil*, ii. 84.

no more than a few bubbles of gas came over, and that only when the lead, by being kept over the fire, had acquired a temperature about its fusing point. On restoring this temperature by adding fresh metal, the evolution of gas was always suspended. I placed also one of Mr. Wedgewood's pyrometer pieces in contact with a retort which was at work at Mr. Lee's manufactory, and which showed only a dull red or blood-coloured heat; but, after remaining in that situation half an hour, a contraction of barely one degree of the scale had taken place. This temperature, however, I suspect is rather too low, and has a tendency to distil over too much tar, and consequently to produce less gas than might be obtained by a degree of heat somewhat higher. The best adapted temperature will probably be found to vary with different kinds of coal; and I have been prevented from ascertaining it with respect to cannel by the inconveniences that would arise from disturbing the regular arrangements of a large manufactory. From some experiments of Mr. Brande, it appears that the sudden application of the requisite heat evolves from coal much more gas than the gradual heating of a cool retort up to the point of ignition.\*

In the experiments upon gas from Wigan cannel, the results of which are comprised in the first table, azotic gas was found in all the aëriform products, from the beginning to the end of the operation. But in experiments on the gas obtained at other times from the same substance no appreciable quantity of azotic gas could be discovered till after the sixth hour of the process, when it began to appear, and progressively rose to 20 parts in the hundred. Of this purity of the early products from azote, and appearance of it in the latter ones, Mr. Dalton was an eye witness on one occasion, when he was so good as to co-operate with me; and I had afterwards repeated opportunities of verifying the fact. With the view of ascertaining whether the azote found its way from the atmosphere into the distilling vessels, I subjected 100 gr. of cannel coal to heat in a glass retort, the capacity of whose body and neck did not together exceed  $1\frac{1}{4}$  cubic inch. Besides a portion of gas which was lost, 50 cubic inches were collected, which, on careful analysis, were found to contain five cubic inches of azotic gas. Of these, only one cubic inch can be traced to the common air present in the retort at the outset; and the other four cubic inches must have been furnished by the coal itself.

It is reasonable indeed to expect that a substance like coal, which affords ammonia under some circumstances, should, under others, yield the elements of that alkali in a detached state; and the reason why azote is for the most part not to be found in the gas which is first evolved is, that at a low temperature that element unites with hydrogen, and composes ammonia.

\* *Journal of Science*, vol. i. p. 75.



But when the contents of the retort, which, for some time, have been kept comparatively cool by the escape of condensable fluids, become more intensely heated, ammonia is either not formed, or, if formed, is decomposed again into azotic and hydrogen gases, both of which may be traced in the æriform products of the advanced stages of distillation. As a matter of practice, it is certainly desirable that the azote existing in coal should enter into the composition of a condensable fluid rather than that it should escape in a gaseous state; for it is an impurity which, when once mingled with the combustible gas, cannot be removed by any known method, and must materially impair its illuminating power. That such an effect must result from its presence may be inferred from the experiments of Sir H. Davy, who found that an explosive mixture of carburetted hydrogen and common air was deprived of its combustibility by being mixed with one-sixth of its bulk of azotic gas.\*

(To be continued.)

#### ARTICLE IV.

*Observations on the Relation of the Law of Definite Proportions in Chemical Combination, to the Constitution of the Acids, Alkalies, and Earths.* By John Murray, M.D. &c. &c.

(Concluded from p. 294.)

THE composition of the acids, of which phosphorus is the base, is so imperfectly determined, and the most recent experimental researches are so much at variance in their results, that scarcely any satisfactory application of a principle can be applied to them. There is some reason to believe that the three acids which appear to be of definite composition, the hypophosphorous, phosphorous, and phosphoric acid, contain oxygen in proportions affording the multiples 1, 2, 4. The intermediate multiple of 3 is probably to be found in the combination which is established of phosphorous acid acting on a base, conformable to the view illustrated in the analogous case of sulphurous acid, the acid receiving the oxygen of the base, and a ternary compound being formed, in which the whole oxygen and the radical of the base observe the due relation to the radical of the acid. And from the quantity of base which phosphorous acid must saturate, this additional proportion of oxygen will be precisely a multiple of that with which phosphorus combines. Phosphoric acid appears to be formed in the combustion of phosphorus in oxygen, and must, therefore, be admitted to exist as an insulated

\* On the Safety Lamp, p. 80.

binary compound. It is further capable, however, of combining, according to the common expression of the result, with a definite proportion of water; that is, with an additional proportion of oxygen, and with hydrogen equivalent to that proportion. The quantity of this has been variously estimated, and does not appear to be very accurately determined; but it will probably be equal to an additional multiple of oxygen, that is, about 14 in 100, and then the series of phosphoric compounds will contain oxygen in the ratio of 1, 2, 3, 4, 5. If the estimate, however, by Berthollet and Berthier were correct, which makes the quantity of combined water equal to 25 in 100, it would be equal to 2 multiples; and the series might be 1, 2, 3, 4, 6. And if phosphorous acid does not combine directly with the elements of the alkaline bases, but forms, as has been affirmed, partly phosphates, partly phosphites, the series will be that of 1, 2, 4, 6, similar to that of the nitrous compounds.

In the muriatic compounds, no regular progression has been discovered, considering either muriatic acid, or chlorine, as the first of the series. Some such progression may, perhaps, however, be traced.

Considering muriatic acid as a compound of a radical with oxygen, Berzelius has inferred, from the application of the principle, that the quantity of oxygen in an acid is either equal to, or a simple multiple of, the quantity of oxygen in a base which it saturates, that it consists of 41.632 of radical, and 58.368 of oxygen. This applies, however, to what is called the real acid free from water, a compound, the existence of which is not proved. Taking the proportion of water, or rather of its elements in hydromuriatic acid into calculation, it gives as the composition 31.224 of radical, 65.851 of oxygen, and 2.925 of hydrogen. The proportion of oxygen to the radical in oxymuriatic acid is the same; the only difference between the two being the presence of hydrogen in muriatic acid; in oxymuriatic acid, therefore, the proportion is 32.164 of radical, with 67.836 of oxygen. The next compound is euchlorine, composed of 100 of oxymuriatic acid, with 22.26 of oxygen; this is almost exactly the third of the former; the relation is, therefore, that of 3, 4. Another gas, which has since been discovered by Sir. H. Davy, contains a much higher proportion of oxygen, being composed of 100 of oxymuriatic acid with 89; this is exactly 4 multiples, and gives, therefore, the series of 3, 4, 7. Hyperoxymuriatic, or chloric acid, is composed of 100 of oxymuriatic acid, with 111 of oxygen, which is another multiple, or 8. It cannot, however, exist insulated, as Gay-Lussac states, without the presence of water; that is, it is a ternary compound, containing probably an additional multiple of oxygen, and thus affording the series of 3, 4, 7, 9. If an error of experiment were supposed with regard to the second, or euchlorine, so as to have deviated from the multiple 5, this would afford a series somewhat regular. But



even without assuming this, it is of importance to find in all these, that the proportions are simple multiples of a first quantity. And as the relations of carbon to oxygen and hydrogen, in the composition of the vegetable acids, show the numerous definite proportions in simple multiples in which they combine, so combinations not more numerous may supply the intermediate multiples in the muriatic compounds.

There is a peculiarity in the muriatic compared with the sulphuric and nitric compounds. In the latter, there does not exist any binary compound of the radical with oxygen, in which the proportion of the one to the other is the same as the proportion in which they exist in the ternary compound which they form with hydrogen. There is, therefore, no oxysulphuric, or oxynitric acid. In hydromuriatic and oxymuriatic acids, the proportion of oxygen to the radical is the same, and there is only in the former an addition of hydrogen. Hence the apparent peculiarity of oxymuriatic acid having an excess of oxygen, and the circumstance, that by an addition of hydrogen it is converted into muriatic acid. This, however, is not absolutely peculiar to it, and presents, therefore, no anomaly. The same thing holds in the relation of carbonic and oxalic acids. In both, the same proportion of oxygen to carbon exists; the oxalic acid only containing, like the muriatic acid, an addition of hydrogen. Did hydrogen act with the same facility on carbonic acid that it does on oxymuriatic acid, it would convert it into oxalic acid in the same manner that it converts the other into muriatic acid. And were the attraction of carbon to metals and inflammables more powerful than it is, so as to bring it into ternary combination with them with oxygen, or its affinity to hydrogen equally strong with that of the radical of muriatic acid, its action, in apparently imparting oxygen, would probably be equally energetic as that of oxymuriatic acid.

The constitution of the alkalis and earths, which I have considered as ternary compounds of radicals with oxygen and hydrogen, will be found to exhibit, in conformity to this view, a perfect coincidence with the law of proportions. One or two examples will be sufficient for illustration.

Potassium, in the proportion of 100 with 20.5 of oxygen, constitutes the binary compound denominated dry potash, and which is probably the first degree of oxidation. If, in the ternary compound, which constitutes the alkali in its common state, fused potash as it is named, the additional portion of oxygen is equal to this, or the whole quantity is twice that in the first, conformable to the usual law of proportions, then the quantity of water which will be obtained from the subversion of its composition, and which, according to the common doctrine, is water combined with the alkali, will be 16 from 100 of the fused potash. Now, Berthollet assigned the quantity from experiment at 14, and Sir. H. Davy at from 17 to 19. The mean

of these may be taken at 16, conformable, therefore, to the theoretical application. And this quantity is stated on the authority of Berzelius as the precise proportion. This second proportion of oxygen seems to be established as an insulated binary compound in combination with the radical, as well as in the ternary combination into which hydrogen also enters, if it is perfectly just, what has been asserted, that the excess of oxygen in the product of the combustion of potassium in oxygen is expelled by heat. And if this compound were capable of being acted on by hydrogen (which can scarcely be doubted it is), it would afford another perfect analogy to oxymuriatic acid, as by this action it would be converted into potash, precisely as oxymuriatic acid is by the same action converted into muriatic acid. The facility with which hydrogen is admitted into the binary compound, so as to form the ternary combination, is still greater than the facility with which a similar change is produced in oxymuriatic acid, the addition of water alone producing the effect, converting the peroxide of potassium into potash, and liberating of course the corresponding excess of oxygen.

Sodium combines with a larger quantity of oxygen than potassium does; and, therefore, soda ought (adopting the language of the common doctrine) to contain a larger quantity of combined water, the water being always proportional to a multiple of the oxygen combined with the radical. The fact is accordingly conformable to this, 100 of fused soda affording about 24 of water.

Barium, on the contrary, combines with less oxygen. Sir H. Davy, from indirect results, infers, that 89.7 of barium combine with 10.3 of oxygen. In conformity to the law, therefore, barytes ought to afford less water, which is accordingly the case, 100 of hydrate of barytes, as it is named, affording, according to the estimate of Berthollet, 9 of water, according to that of Berzelius about 10.5.

The neutralization of acids and of oxides, by their mutual action, I have already stated, is probably not merely the result of combination, but of subversion of composition. The radical of the acid, and the radical of the base, are in combination with the oxygen which remains after the abstraction of any portion of this element by the formation of water. And the proportions established will be found directly conformable to the relations of these elements. It has been already shown, that the relation of the oxygen in the ternary combination is that which it separately observes to the radical of the acid, and the relation of the radical of the alkaline base is that which it also separately observes to the radical of the acid. And the three elements exist in simultaneous combination. So far the constitution is analogous to the composition of the ternary acids and bases, with this difference, that in these the oxygen and hydrogen are in their respective proportions to the radical of the acid or base,

and in the salts the oxygen and the radical of the base are in their due proportions to the radical of the acid. In the conversion of the one into the other, there is merely the substitution of the radical of the base for the hydrogen of the acid, and the abstraction of that portion of oxygen with which the former was combined, and the formation of a portion of water equivalent to this. In the formation of a neutral salt from the union of a binary acid, there is simply the production of a ternary combination in which the proportion of oxygen to the radical of the acid is increased by that of the base. And the difference in the salts formed by the binary and ternary acids of the same radical is in the quantity of oxygen being a higher multiple in those of the latter than in those of the former; so that the addition or abstraction of that portion of oxygen converts the one into the other.

There is every reason to infer, that in the ternary acids, and the ternary alkaline bases, while the due relation of oxygen to the radical, and of hydrogen to the radical, exists, there will be a similar relation in the hydrogen and oxygen to each other. These two elements combine only in the proportion of 1 to 7.5. But there may be other proportions multiples or submultiples of these, in which they exert mutual actions, though they do not in conformity to them form binary combinations, and they may exist under the influence of such actions in ternary combinations. In hydrosulphuric acid the quantity of oxygen in relation to the hydrogen present is four times the quantity of oxygen which constitutes the composition of water. And this may be a relation actually existing, independent of the others, that is, while the oxygen in the proportion in which it is present, acts on the sulphur, and the hydrogen acts on the sulphur, the oxygen and hydrogen likewise act on each other; and these actions are in equilibrium constituting the sulphuric acid. And in all these ternary compounds at least, the elements may exist in these uniform relations instead of any of them being in any case in intermediate proportions. In like manner, in the compound salts, the two radicals, that of the acid, and that of the base, will observe their due relation in proportions to each other.

In the neutral salts then there exists neither acid nor alkali; and their decomposition is merely the transfer of the radical of the base in the one to the radical of the acid of the other. The decomposition, for example, of nitrate of barytes by sulphate of potash, consists in the transfer of barium to sulphur and oxygen, and of potassium to nitrogen and oxygen. The quantities must be equivalent to each other; and hence the law of Richter, that the state of neutralization remains.\*

\* Under these principles, the laws given by Berzelius with regard to the quantity of combined water in acids and in bases, and the proportion which the oxygen in an acid bears to the oxygen of an oxide with which it combines, which some have regarded merely as empirical, and which others have denied, are explained.

In the mutual action of acids and salifiable bases with regard to saturation, the simple rule will be, that in all cases an acid will saturate that quantity of a base, the radical of which is in the equivalent weight to the radical of the acid. And the quantity of oxygen in the salt will be that which constitutes the usual proportion of that element to the radical of the acid.

The capacity of saturation in the different acids and bases, in their reciprocal action, has been proposed as a measure of the force of affinity which they exert, those acids being inferred to have the strongest attractions to the salifiable bases, which in the smallest quantities saturate a given weight of these bases; and the same rule being applied to the attraction of the bases to the acids. The capacity of saturation, however, depends altogether on a different cause, on the relations of the more remote elements to each other, and not any direct action of acid and base. A larger quantity of barytes is necessary to saturate a given weight of the different acids than of potash, not because barytes has a weaker action on acids than potash has, but because the combining weight of barium is greater than that of potassium; and it combines, therefore, in larger quantity with the radicals of the acids; and conversely, a larger quantity of sulphuric than of carbonic acid is necessary to saturate a given weight of the different bases, not because its affinity to them is less powerful, but because the combining weight of sulphur is higher than that of carbon. And were the doctrine of the influence of elective affinities, independent of the operation of external forces on chemical attraction established, barytes would be considered as exerting a more powerful attraction than potash to sulphuric acid, from the attraction of barium to sulphur being stronger than that of potassium to sulphur. From the test, however, of the strength of attraction to be found in the capacity for saturation, the attraction of potassium must be inferred to be superior to that of barium to sulphur; and the results of double decomposition of what are called their saline compounds must be ascribed, in conformity to Berthollet's doctrine, to the influence of the force of cohesion, this force acting more powerfully on the ternary compound of barium, sulphur, and oxygen, than on that of potassium, sulphur, and oxygen. These views apply to all the other cases of decomposition in saline combinations.

*Supplement to the preceding Paper.*

Sir H. Davy has stated some experiments in opposition to the evidence of water being procured from the action of muriatic acid gas in metals.\* On these, so far as they refer to the expe-

They follow indeed necessarily from the relations in the combining weights of the elements, when these are considered as in simultaneous combination.

\* Philosophical Transactions for 1818, Part I.



riments which I executed on this subject,\* I may take this opportunity of offering a few observations.

In passing muriatic acid gas through glass tubes ignited, Sir H. Davy found water to be deposited, which he ascribes to the action of the acid on the oxide of lead and alkali in the glass.

In passing it through glass tubes containing iron ignited (the experiment I had performed), "much more water appeared;" but this he ascribes principally to the "combination of hydrogen disengaged from the muriatic acid gas by the iron, with the oxygen of the common air." Any one repeating this experiment will at once be satisfied that this circumstance can have little or no effect in producing the result. The water does not appear until the air has been expelled from the tube by the introduction of the muriatic acid gas; it continues to increase after this when no air can be supposed present; and the whole quantity of air which the tube could contain were it even filled with it, is inadequate to afford by its oxygen any sensible production of water in such an experiment. The circumstances and result of the experiment which I have described, p. 297, of the eighth volume of the Transactions of the Royal Society of Edinburgh, in which the air in the tube had been previously expelled by the introduction of the gas, and that described, p. 298, in which the air had been withdrawn from the retort by previous exhaustion, altogether preclude this supposition. But its utter inadequacy will, to any one taking the trouble of repeating the experiment, be sufficiently apparent.

It is stated that in the action of muriatic acid gas on metals, hydrogen, equal in bulk to half the volume of the gas, is produced; and, therefore, it is added, if water had been generated by the action of muriatic acid gas on metals, it must have been the chlorine, or the metal, or both, that were decomposed. "But in an experiment of passing chlorine gas over ignited metals, not the slightest appearance of moisture was perceptible."

This argument, in common with some others of Sir H. Davy's results, may apply with sufficient force to those experiments in which it is said, that water was obtained equal or nearly so to the whole quantity of water, which, according to the old doctrine, is contained in muriatic acid gas; for it is evident that this water could not have been deposited, and hydrogen likewise evolved. But it does not apply to my experiments, in which a small though very sensible portion of water was obtained; for in such a case hydrogen gas will also be produced, though not to the precise amount of half the volume. The actual result, therefore, in the particular form of experiment employed, ought to be ascertained, instead of a general conclusion being reasoned on, more especially when even the general fact is not so clearly

\* Edinburgh Philosophical Transactions, vol. viii. p. 287.

established that it can be held as demonstrated. The theoretical result no doubt is that hydrogen will be evolved equal to half the volume of the muriatic acid gas, since the latter is formed from equal volumes of hydrogen and chlorine. But circumstances may occur connected with the experiment which will modify this. There is one obvious circumstance of this nature, that of the absorption of a portion of the muriatic acid gas by the muriate formed, whence, as the entire quantity of acid is not decomposed, the quantity of hydrogen produced must, if the experiment be accurately performed, appear less than half the volume. On this point accordingly there has been considerable diversity of result. Sir H. Davy himself, at a former period, in experiments conducted with much care, and having no reference to theory, found that the quantity of hydrogen evolved from the action of potassium and of mercury on muriatic acid gas is equal only to about one-third of the original volume of the gas.\* When, therefore, the conclusion is adopted as the ground of argument that the quantity is one-half without any allusion to any difficulty in the experiment, any source of fallacy attending it, or any opposite result having been obtained, its inaccuracy, or at least its uncertainty, may be fairly presumed. I had already observed, in relation to this point, that if the whole water essential to the acid is decomposed by the action of the metal, half the volume of hydrogen ought to be obtained, muriatic acid gas being composed of equal volumes of oxymuriatic gas and hydrogen gas; while, on the other hand, if any additional portion of acid enter into union, besides that forming a neutral compound, the water of this will be liberated, and of course the full proportion of hydrogen will not be obtained. I, therefore, endeavoured to determine whether this is the case or not. And in repeated experiments, in which iron and zinc acted on muriatic acid gas, the quantity of hydrogen was always less than the half; and on an average, about 12 measures were obtained when 30 measures had been consumed.\* It appears, therefore, that in experiments attended with the results I had obtained; that is, when a portion of water is obtained from the action of metals on muriatic acid gas, and a supermuriate is formed, the quantity of hydrogen evolved is *not equal* to half the volume of the gas consumed; and hence, in reference to these experiments at least, Sir H. Davy's attempt to decompose chlorine was very unnecessary, and the want of success, which it was easy to anticipate, affords no argument whatever.

Muriate of ammonia, it is stated, is not altered by being passed through porcelain or glass tubes heated to redness; but if metals be present, it affords similar results to muriatic acid gas; and the water obtained is ascribed to the action of the hydrogen liberated from the acid and from the ammonia, on the action of

\* Philosophical Transactions, 1809, 1810.

† Transactions of the Royal Society of Edinburgh, vol. viii. p. 307.



oxide of lead in the glass. In the experiments of which I have given an account (*Edinburgh Transactions*, vol. viii. p. 301), I found that exposure to a heat, not so high as that of ignition, is not necessary to obtain water from the action of metals on muriate of ammonia; one much more moderate, and at which no action on the glass can be supposed to be exerted, is sufficient; and accordingly, not the slightest indication of the glass being acted on, can be perceived. In obtaining, for example, water from a mixture of tin filings and muriate of ammonia, heated in a retort by the gentle heat of a small lamp, the retort remains perfectly unaltered in colour, transparency, and lustre.

These objections then I regard as of no force. At the same time I do not consider the discussion as of much importance. The view which I have now proposed of the nature of muriatic acid does not rest on any exclusive proof of water being obtained from it but on other grounds. And it is quite sufficient that it yields water in the same cases of chemical action, in which other powerful acids, as the sulphuric, nitric, and oxalic, afford it; while the sulphurous and carbonic afford none. The same theory applied to the constitution of the former will fall with every probability to be applied to that of muriatic acid; and whatever superiority may belong to it, this will be applied to both. The question, therefore, deserves attention only on the principle that in chemical investigations it is always of importance to adhere rigidly to the observation and strict expression of a fact whether it is conformable to a prevalent doctrine or not, or whether it admits of obvious explanation or not on any established law. In numerous experiments on muriatic acid gas, I have always obtained water in small but very sensible quantity, where its production, I am satisfied, cannot be accounted for from any of the extraneous sources to which it has been attempted to refer it. And I certainly shall not refrain from maintaining what I regard as the strict expression of an experimental result. At the same time, in the experiment at present referred to, the formation of a supermuriate affords a principle which, as I have already stated, sufficiently accounts for the fact.

## ARTICLE V.

### *On a new Acid formed of Sulphur and Oxygen.\**

By MM. Welter and Gay-Lussac.

THE acid, which constitutes the subject of this memoir, lies, as far as the proportion of its elements is concerned, between

\* The origin of this acid is as follows: While M. Welter managed a bleaching work, he made sulphurous acid to act upon the black oxide of manganese, which

sulphurous and sulphuric acid ; but it differs from them exceedingly in its properties and in its mode of composition, which does not resemble that of any other acid. We shall distinguish it provisionally by the name of *hyposulphuric*, from the analogy of the hyposulphurous acid, in order to indicate that it contains less oxygen than sulphuric acid, and more than sulphurous acid. Its salts will take the name of *hyposulphates*.

Hyposulphuric acid is formed when we pass sulphurous acid gas into water, holding peroxide of manganese in suspension. The combination takes place immediately, and we obtain a solution perfectly neutral composed of sulphate and hyposulphate of manganese. The hyposulphate of barytes being soluble, we decompose these salts by barytes, which is added in excess. A current of carbonic acid gas is then passed through the liquid to saturate the excess of barytes, and, by heating, to disengage the carbonic acid, which retains in solution a small portion of carbonate, we obtain the hyposulphate of barytes. To obtain this salt quite pure, it is proper to crystallize it, because it may contain lime, from which the peroxide of manganese is not always free. By decomposing this salt with sulphuric acid to perfect saturation, we obtain hyposulphuric acid in an uncombined state.

This acid has no smell even when as concentrated as possible. Its taste is decidedly acid. It does not seem capable of assuming the state of a permanently elastic fluid. When placed under the vacuum of an air-pump along with sulphuric acid at the temperature of  $50^{\circ}$ , it becomes concentrated without being sensibly volatilized. When it reaches the specific gravity of 1.347, it begins to be decomposed ; sulphurous acid is exhaled, and sulphuric acid remains. When heated in a very diluted state, it gives out pure water ; but by and by sulphurous acid is disengaged, and sulphuric acid produced. The water-bath is sufficient to produce this decomposition. While cold, it is not altered by chlorine, concentrated nitric acid, or the red sulphate of manganese. It saturates the different bases, and forms soluble salts with barytes, strontian, lime, oxide of lead, and probably with all the bases. It dissolves zinc with the disengagement of hydrogen without being decomposed. It contains two proportions of sulphur and five of oxygen, and a certain quantity of water which appears essential to its existence when not united to a base. It was by the analysis of the hyposulphate of barytes that we were led to that of hyposulphuric acid.

This salt forms brilliant crystals in four-sided prisms terminated by a great number of faces. It undergoes no alteration

he employed to procure chlorine, and he remarked, contrary to the received opinion, that there was formed a neutral bisulphate, which he conceived to contain peroxide, as its base. He mentioned this fact to me, and requested me to examine it. We united together in my laboratory at the *direction des poudres*, to examine the subject.—G.-L.

in the air, nor even in a vacuum dried by sulphuric acid. One hundred parts of water, at the temperature of  $46\frac{1}{4}^{\circ}$ , dissolve 13.94 parts of this salt. The solution is not altered by chlorine. The crystals decrepitate strongly. A moderate heat is sufficient to decompose them; water and sulphurous acid is disengaged; and there remains a neutral sulphate of barytes: 100 parts of the hyposulphate well dried in the air lost 29.903 by calcination; and consequently left 70.097 of sulphate of barytes: 100 other parts of the same salt mixed with the chlorate and carbonate of potash, and heated to redness in a platinum crucible, gave, after being precipitated by the chloride of barium, and properly washed, 138.300 of sulphate of barytes. This number is not exactly the double of 70.097; but as it is very difficult not to lose a little of the sulphate of barytes during the washing, we shall admit that the second number ought to contain the first number exactly twice. On this supposition, the hyposulphate of barytes may be considered as formed of one proportion of barytes, one proportion of sulphuric acid, and one proportion of sulphurous acid; and if we calculate the ratio of these elements, taking 5.0 for sulphuric acid, 4.0 for sulphurous acid, and 9.7 for barytes, we find for 100 of the hyposulphate 70.12 of sulphate of barytes, instead of 70.097. The quantity of water may be determined from the difference between the weight of the salt and that of the sulphate of barytes and sulphurous acid extracted from it. We find from this analysis that the hyposulphate of barytes is composed of

1 proportion of barytes. ....	9.700
1 proportion of sulphuric acid. ....	5.000
1 proportion sulphurous acid. ....	4.000
2 proportions of water. ....	2.264

Or of

1 proportion of barytes. ....	9.700
1 proportion of hyposulphurous acid. ..	9.000
2 proportions of water. ....	2.264

Consequently the hyposulphuric acid, which neutralizes one proportion of base, is formed of

2 proportions of sulphur. ....	4.0
5 proportions of oxygen. ....	5.0
and its equivalent number is. ....	9.0

Thus we have an acid which neutralizes the bases very well, and whose bases remain in the state of neutral sulphates on losing one proportion of sulphurous acid. It contains the same proportion of sulphur as hyposulphurous acid, and  $2\frac{1}{2}$  times as much oxygen. These two acids must constitute a particular group among the acids of sulphur. Sulphurous and sulphuric acid will form another. This distinction is necessary, because





The oxide of manganese made by means of chlorine hardly gives any hyposulphate. Perhaps the oxide which we employed was not at a maximum of oxidation; and probably in this respect there is a great difference between different oxides of manganese. We have not been able to obtain hyposulphuric acid by treating with sulphurous acid the hydrated peroxide of barium, or the brown oxide of lead, though these two oxides present a composition analogous to that of peroxide of manganese.

We shall conclude by stating the essential characters of hyposulphuric acid, and of its salts.

Hyposulphuric acid is distinguished from the other acids of sulphur by the following properties:

1. It is decomposed into sulphurous and sulphuric acids when exposed to heat.
2. It forms soluble salts with barytes, strontian, lime, lead, and silver.

The characters of the hyposulphates are:

1. They are all soluble.
2. They only yield sulphurous acid when their solutions are mixed with acids, if the mixture becomes hot of itself, or be artificially heated.
3. They disengage a great deal of sulphurous acid at a high temperature, and are converted into neutral sulphates.

## ARTICLE VI.

### *On propelling Vessels by Means of Windmill Sails.*

By J. M. Bartlett.\*

It is but reasonable to conclude that the honour of inventing steam-boats (now no less a subject of controversy between individuals than nations) will soon cease to occupy the attention of mankind; for since experience proves their inutility, except to a very limited extent, reason very naturally rejects their adoption. Nor is it to be wondered at; for, whilst the expansibility of steam renders it a powerful agent, it makes it, at the same time, a very dangerous auxiliary to the wants of man.†

But it is not the insecurity alone, attendant on its operations, which lessens the importance of steam, as a propeller of floating bodies; many causes combine to render it either inefficient or impracticable. The *fuel* which would be required for a vessel of

\* This paper was published before in the Pamphleteer. We have been induced to insert it here that it may be better known. The idea is not new. It was proposed nearly a century ago; but I do not know whether it has been ever tried.—T.

† See the Minutes of the Evidence respecting Steam-boats before a Select Committee of the House of Commons, in 1817.

any considerable burden, to perform a moderate voyage even, would leave but little space for freightage! However extraordinary this may appear, it is nevertheless indisputable; to prove which, I will instance a boat of 80 tons (said to be the most perfect of its kind) which navigates the Clyde, propelled by an engine of 14 horse power, and which consumes  $1\frac{1}{4}$  cwt. of coals per hour! The quantity, therefore, which would necessarily be required for a ship to perform, as stated, a moderate voyage only, is almost incalculable, particularly as the increase of velocity is not in proportion to the increase of the power of the steam-engine; for the resistance, to which a boat is subject, increases not in an arithmetical proportion, but in proportion to the squares of velocity; in other words, to make the same vessel move with 10 times a given velocity, it requires 100 times the power.\*

Again, it is known how ungovernable a steam-vessel is, when any part of the machinery chances to be deranged; when, therefore, its complication is considered, and *the vessel's dependence upon that power alone*, the danger of navigating oceans like the Atlantic or Pacific, becomes multiplied in a ratio, equal to the distances of the ports of departure or destination. In fact, numberless causes may occur to retard a vessel's progress so propelled; so that steam, in this instance, can derive but little value from the circumstance of its being independent of the operations of winds and tides.

Indeed, the propulsion of ships of any considerable magnitude, by that power, to answer a profitable purpose, seems no longer tenable, even upon paper; and, however theoretically ingenious, we can only lament that it is not more practically useful.

The object of the present address is to suggest the employment of windmill sails as substitutes for steam, in giving motion to the paddles: by this means a power, at least equal to that of steam, may be obtained; and as those sails are at all times capable of being turned to the wind, to receive its impulse, the advantages of sailing against wind and tide will be retained, the cost of machinery very considerably lessened, the expenditure and inconveniences of fuel saved, and the danger, in comparison with steam, rendered as nought.

Of the practicability of the measure, I conceive it only necessary for a person (however ignorant he may be of the mechanical power of the lever) to witness the sails of a windmill in motion to be convinced; but an accession of force would be obtained by the employment of this power to the purposes of navigation, which, if I am not mistaken, is new in physics—I mean the excess of velocity which would be acquired by progression, thus constituting power multiplied by power; for no sooner would the *vis inertiae* of the body to be propelled be overcome, than the

\* See Rees's Ency. Art. Steam-boat.



sails, I apprehend, would derive an additional impetus from the vessel's velocity.

Whether or not this hypothesis be founded on a just datum, I am not prepared to dispute, nor is it necessary to my purpose to prove an acceleration of motion; for, as the sails of a windmill move with a velocity nearly equal to the squares of the velocity of the wind, a maximum of effect may be produced equal to the end proposed.

To pursue the subject of the address; and which I shall do more by reference to a power familiar to all than by any mathematical demonstration of mere speculative opinion, I assume (and it is not too much) that one set of common windmill sails is equal to a steam-engine of 20 horse power; consequently is capable of propelling a vessel of 120 tons (as that power has been found to do) at the average rate of six or seven miles per hour, against wind and tide. If, therefore, one set impart a momentum equal to the propulsion of a vessel of moderate burden, three sets, placed longitudinally, as the masts of a ship, and acting independently of each other, with corresponding water-wheels, must impart momenta equal to a steam-engine of 60 horse power, or the propulsion of a ship of very considerable dimensions. This is but a common inference deduced from known and acknowledged data, and which will, I conceive, render the proposition self-evident. But, supposing the means proposed to be inadequate to the end, an excess of power may be obtained; as I will presently show, by the scientific application of those means, which would greatly exceed any sum of force which might be required. The extremely rude construction of the windmill sails in common use has often attracted the attention of men of science; and the only way to account for that species of mechanical power remaining unimproved for centuries is, that a quantum of force was probably thereby acquired sufficient for the purposes for which it was employed.\* That this power might be increased, I will cite the following, out of numerous authorities, to prove; at the same time I beg it to be understood, that it is far from my intention (although the subject may almost demand it) to swell this paper into an essay on windmill sails. Ferguson† very ingeniously suggests, that as the end of the sail nearest the axis cannot move with the same velocity which the tips, or further ends, have, although the wind acts equally strong, a better position, perhaps, than that of stretching them along the arms directly from the centre of motion, might be, to have them set perpendicularly across the further ends of the arms, and there adjusted lengthwise to the proper angle; for in that case both ends of the sails would move with the same

\* According to Ferguson this is the case; for he shows that if the stones of a mill revolve more than 70 times per minute, they pulverize the bran with the flour.

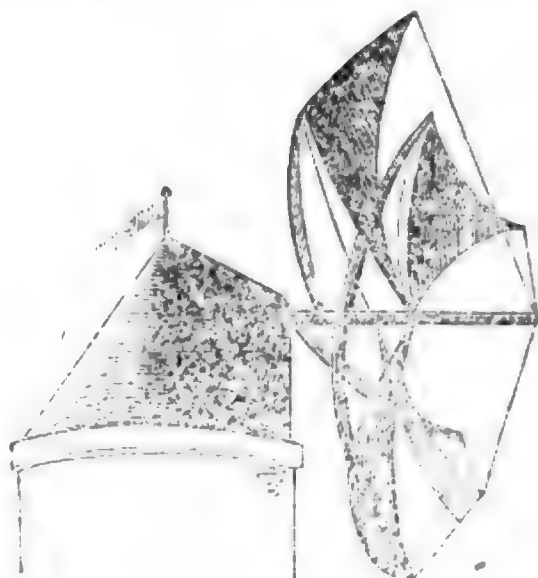
† Lect. on Mech. p. 52.

velocity, and being further from the centre of motion, they would have so much the more power. M. Parent, however, considers that the figure of the sails of a windmill should be elliptical to receive the greatest impulse from the wind; and a windmill with six elliptical sails, he shows, would have more power than one with four only, as the force of the six would be greater than that of the four in the ratio of 245 to 231.

The same author also considers which form among the rectangular sails will be most advantageous, i. e. that which shall have the product of the surface by the lever of the wind, the greatest. The result of this inquiry is, that the width of the rectangular sail should be nearly *double its length*; whereas usually the length is made almost five times the width!\* The following peculiar construction of the sails of a windmill used in the vicinity of Lisbon, merits notice from its being, according to the opinion of Lord Somerville, superior to those used in Great Britain. Their advantages are thus detailed by his lordship:

1. The broad part of the sail is at the end of the lever, and thus an equal resistance may be overcome with less length of arms.
2. The sails, constructed upon this plan, may be set to draw in a manner similar to the stay-sails of a ship; and as they are swelled more than those of common mills, they render it unnecessary to bring the mill so frequently to the wind—a practice attended with considerable trouble. The annexed sketch will, perhaps, convey a better idea of its utility; and at the same time of its applicability to nautical purposes.

Windmill sails may also be made to act horizontally. A scientific mill was, some time since, constructed at Battersea, on the principle, I conjecture, of the wind-towers of the Asiatics, only that a number of horizontal sails revolved



\* For the following calculation respecting elliptical sails, I am indebted to a friend. "If the sails of a windmill form a complete ellipsis, whose transverse diameter is 80 feet, and conjugate 64, and are so disposed that the conjugate, or rather semi-conjugate, forms the length of the arms, and by this disposition receives the whole force of the wind, and loses none, it is then to be observed that these sails embrace a surface, or rather present to the wind a surface of 4,021,248 square feet; then, admitting the wind to be brisk, or what nautical men term 'a snug breeze,' the wind at that rate acts with a force of about a pound on each square foot, or 4021 lbs. on the six sails combined; or supposing, for argument, the dimensions or surface of the sails equal, 670,208 lbs. on each sail. Now, as the sails are a lever of the first order, of course the power of each sail is in proportion to the length of the lever (or the circumference described by it), compared with the semi-axis (or

around the same shaft.\* *Query.* Might not those sails, or even those of the Portuguese windmill, be advantageously employed as auxiliary means (should such be found requisite to propel ships of the greatest dimensions) as the stud or stay-sails, &c. of ships are in light breezes? and instead of being confined to three rectangular, or elliptical sails, might not the number be increased to embrace a surface nearly equal to the present sails of a ship when set? Independently of those means, or the improvement of the common rectangular sail, a considerable accession of force, I am convinced, might be obtained by an attention to the construction and more scientific application of the paddles themselves. The present form was adopted in the infancy of the invention of steam-boats; and, although numerous experiments have been tried without, unfortunately, any practical good having resulted from them; it is yet evident that much remains to be done. From the circumstance of half the wheel only being submerged at any one time during its action, it follows that the wind must oppose a very considerable resistance to its rotatory motion; the effect of which may be more easily conceived when it is known that it performs upon an average 40 revolutions per minute. Hence an incalculable advantage would be obtained could the paddles be brought to present a smaller surface to the retarding force of the air, similar to the oars of a boat, which are said to be feathered when their edges alone are opposed to the wind during the interval of the strokes. As my present proposition is to apply another power to the machinery in use, I will trust to experience more matured to suggest a remedy for this defect in the mechanical propulsion of vessels by steam or otherwise. A few of the advantages which this plan, if adopted, would possess over steam, have been already detailed; that it would possess as great a superiority over the present mode of navigation must be equally evident; for, whilst it would share with steam the singular advantage of sailing against wind or tide, whereby navigation may be rendered comparatively safe, the simplicity of the method proposed would render it infinitely preferable to both. Masts, sails, ropes, spars, &c. form no inconsiderable share of the sum total of a vessel's cost; and to a maritime nation, they become of national importance, when derived from foreign sources.

What the author now submits to the public is at best but a hasty sketch. He has merely embodied those ideas which

(its circumference); therefore allowing the circumference of the axis to be  $\frac{1}{32}$  of the circumference of the circle described by the revolution of a sail, in that case, each sail will have the power of  $670,208 \times 32 = 21,446,656$  lbs. and the whole ellipse six times that; viz. 128,679,936 lbs. or capable of removing  $574\frac{1}{2}$  tons, exclusive of friction."

\* See Dr. Gregory's Dictionary of Arts and Sciences; Art. Windmill.

rapidly occurred to him on a first view of the subject; and as his only object is to court an investigation of a plan which, if successful, must tend, in so great a degree, towards the advancement of the interests and happiness of mankind, he sincerely trusts that it will not be deemed altogether undeserving of experiment.

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## ARTICLE VII.

*Method of making a fine Purple Colour for painting in Oil.\**  
By his Excellence the Count Le Maistre, of St. Petersburg.

(To Dr. Crichton.)

SIR,

St. Petersburg, May 19, 1819.

I now send you the details which you requested respecting my experiments on the oxides of gold. My object in making them was to procure an unalterable purple colour, which could be employed in oil painting, and which should be equally beautiful with that of the purple of cassius when it is fused on enamel or porcelain.

Painters have often tried to employ the purple of cassius with oil or with water; but when mixed with oil, it is destitute of a body, and gives dirty and disagreeable colours. It is used with gum for dark shades, and in the same way as the common lacs mixed with a little black without ever obtaining a purple colour.

The oxide of gold in its nitro-muriatic solution has a natural disposition to pass to purple. Not only tin gives it that colour, but its combination with gelatin, with starch, and with several of the earths, has the same shade of colour.

If we boil a weak solution of starch with a few drops of the nitromuriate of gold, we obtain a precipitate similar in colour to that of cassius; but it retains its purple colour only while moist, and becomes violet when dry. Size (*la colle de gand legere*) mixed in small quantity with the solution of gold becomes purple after being exposed for some days to the air.

In like manner, a dilute solution of gold mixed with different earthy salts, and precipitated by carbonate of soda, furnishes mixtures of the earths and oxide of gold, which become purple when exposed to the requisite degree of heat.

This is the principle upon which the composition of the new colour is founded. After numerous trials with different kinds of earths and their mixtures, I observed that the combination of gold with alumina, when heated sufficiently, gave by far the

\* I am indebted for this valuable paper to Dr. Crichton, Physician in Ordinary to the Emperor of Russia.





finest shade, and that the more concentrated the solution of the alumina was the nearer did the shade approach to purple.

The oxide of gold thus precipitated with the earths combines with them in different manners, according as the solutions are more or less diluted with water. When the sulphate of alumina is dissolved in a great deal of water, the precipitate of gold and alumina becomes bluish, and sometimes rose red, when dried; and when exposed to the fire, produces only a violet colour. If, on the other hand, we dissolve alum in as little water as possible, the auroaluminous precipitate is yellowish, and becomes purple when exposed to the action of heat. I found in the course of these experiments, that sulphate of barytes, when mixed with the alumina, gave it a body, and increased the lustre of the colour. In consequence, I finally adopted the following process:

One part of dry muriate of alumina, one part of sulphate of magnesia, four parts of muriate of barytes, and five parts of carbonate of soda, are each pulverized separately. The pounded salts are mixed in a glass mortar, and a little water is added, merely enough to moisten the mixture. Then a diluted solution of gold is added by little and little, pounding the matter all the time in the mortar, till the whole has acquired a light sulphur yellow tint, and the consistence of cream. The pounding is continued a long time to produce the decomposition of the salts with as little water as possible. When no more effervescence is perceptible, and when the salts cease to creak under the pestle, a sufficient quantity of water is to be added for the complete solution of the salts. This tedious process is essential to unite the oxide of gold with the earths, and the whole success of the operation (which is pretty capricious) depends upon it. The precipitate is to be left for 24 hours in the mortar, stirring it from time to time with a glass rod. It is then to be poured into a saucer, or other similar vessel, and left till the powder subsides. The supernatant liquid is then drawn off with a syphon. The deposit is then dried in the shade without washing it.

The specimen (*a*), which accompanies this letter, was made in this way. I have employed it in painting the draperies of small pictures.

The precipitate, when dried, is yellowish-white. The muffle in which it is to be baked ought to be red-hot. The powder is put upon a silver or porcelain plate, of the thickness of one or two lines; and it must be withdrawn from the fire the instant that it acquires its purple colour. If it be left too long exposed to heat, it acquires a tinge of violet. This is occasioned by the salts which it still contains; for after it has been washed, it may be kept red-hot without losing any of its colour, which indeed acquires greater lustre. These trials were made on a small scale, and are certainly susceptible of being improved, by examining with more care the proportions of salts which should be employed. Though this lake appears to want intensity, the

mixture of oils or gum renders it sufficiently dark ; and experience has shown that it will answer for all the occasions which painters can require.

I have in vain attempted to obtain a colour of greater intensity by increasing the proportion of gold. The shade acquires more of the violet as it becomes deeper, as may be seen in the specimen (b), which may be employed with advantage in painting the shades. In general, the violet-purple tints are very easily obtained ; all the earths furnish them ; but it is very difficult to have the true purple of the specimen (a) by any other process than the one which I have described.

For oil painting, this colour must be carefully rubbed with a mixture of drying oil and varnish. The painting is to be begun by a thin transparent coat. A second coat is sufficient to give it all the lustre of which it is susceptible—a lustre which is equal to the ordinary cochineal lake. The under coats (*les dessous*) ought to be prepared with rough terra de Sienna.

This unalterable colour is particularly useful in miniature painting, and may be used instead of cochineal lakes in carnations. A mixture of it with vermilion gives beautiful tints ; and light, which gradually destroys the light shades of carmine, has no effect whatever upon gold purple, which is capable of resisting both the action of fire and of light.

A detailed memoir of these experiments was presented in 1818 to the Academy of Sciences of Turin, and it has been ordered to be printed in the last volume of its collection of memoirs.

Accept, Sir, the assurance of the most distinguished consideration with which I have the honour to be, Sir,

Your very humble and obedient servant,

The COUNT DE MAISTRE.

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## ARTICLE VIII.

*Some Observations on the Action of Nitric Acid on Lithic or Uric Acid, in Reply to M. Vauquelin.* By W. Prout, M.D. F.R.S. &c.

UPWARDS of twelve months ago, I was informed that M. Vauquelin had been repeating my experiments on lithic acid, and had not been able to succeed in forming purpuric acid. Since that time, I have been waiting with some degree of impatience to see what this celebrated chemist had to say on the subject. At length a notice has appeared in the last number of the Royal Institution Journal, to which I now purpose briefly to reply.

In the first place, M. Vauquelin denominates me a *repeater* of M. Brugnatelli's experiments—a term calculated to convey an



erroneous impression. I did not repeat M. Brugnatelli's experiments. My experiments were made, and my paper on the subject read at the Royal Society, before I had seen or even heard of M. Brugnatelli's Essay.\* Secondly, the *erythric* acid of M. Brugnatelli differs altogether in its properties from the purpuric acid—a substance which M. Brugnatelli had never supposed to exist, as any one may convince himself who chooses to consult his observations. What I consider as the *erythric* acid of M. Brugnatelli may be thus formed: Dissolve pure lithic acid in a slight excess of nitric acid: evaporate the solution slowly, and put it by to crystallize in a warm place. Transparent colourless crystals will be speedily formed, having all the properties ascribed to them by M. Brugnatelli. These crystals are also formed when purpuric acid or purpurate of ammonia is dissolved in nitric acid, and treated in a similar manner. I am not quite satisfied with respect to their composition, but at present I consider them to be a quadruple salt, formed by the union of super-nitrate and superpurpurate of ammonia; or they may be a simple compound of nitric and purpuric acid. This salt I had very frequently formed, but had not attended much to its properties. I had also recognized the formation of another principle which was probably the same as that described by M. Vauquelin. This latter, however, I had not examined at all. I allow, therefore, to M. Brugnatelli, and especially to M. Vauquelin, the merit of having discovered and described the principles respectively claimed by them; reserving only to myself the discovery of purpuric acid—a principle differing distinctly from both the others, the curious and interesting properties of which absorbed my whole attention, and prevented me from extending my researches further at that time. I intended indeed to extend the investigation further at some future time, and have in fact done so; but not being quite satisfied on some points, I shall not enter upon the subject at present.

I am sorry M. Vauquelin has gone so far as to deny the existence of purpuric acid. To convince him of his error, I thought the shortest way would be to give him an opportunity of seeing the substance and of examining its properties. Accordingly I availed myself a few days ago of an opportunity of transmitting to him a small quantity of purpuric acid, purpurate of ammonia, and pure lithic acid.

In conclusion I may remark that I attribute M. Vauquelin's want of success to his operating upon an impure lithic acid. It is difficult to obtain purpuric acid from the lithic acid constitut-

\* My paper on purpuric acid was read at the Royal Society, June 11, 1818. The translation of M. Brugnatelli's paper did not appear in the *Phil. Mag.* (see the number for July, 1818, vol. 52) till the following month, which was the medium through which I became acquainted with M. Brugnatelli's observations. The original paper, which I have not yet seen, appeared, I believe, in the *Giornale di Fisica* for the months of Jan. Feb. March, and April, preceding.

ing human calculi, even when purified as well as possible in the manner commonly recommended. I always used in my experiments pure lithic acid, prepared from the excrements of the serpent exhibited as the boa constrictor, from which purpuric acid may be readily obtained by the method pointed out in my paper.

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## ARTICLE IX.

### ANALYSES OF BOOKS.

*A Critical Examination of the first Principles of Geology; in a Series of Essays.* By G. B. Greenough, President of the Geological Society, F.R.S. F.L.S. London.

(Continued from p. 309.)

#### II. *On the Figure of the Earth.*

THIS essay contains a very learned and elaborate discussion respecting the cause of the inequalities which at present exist upon the surface of the earth—a subject which has been very much discussed by geologists, and respecting which a great variety of opinions have been advanced. If we were disposed to look out for faults in this elaborate essay, which exhibits much learning and much practical knowledge of the structure of the crust of the globe, we would remark, that it is rather deficient in that precise arrangement, that *lucidus ordo* upon which a very great portion of the value of scientific writings depends. The reader, before he can appreciate the value, or even understand the full amount of the information communicated, must be nearly as well acquainted with the subject as the author himself; yet surely it is not for those who are already proficient in geological investigations that these essays are intended; but for those who have not yet made up their minds respecting the great points under discussion, and have not yet enlisted themselves under the banners of any leader. In a science abounding, like geology, in many vague and conjectural speculations, concerning which, absolute certainty being out of the question, we must rest satisfied with ingenious reasonings, or plausible conjectures, it is not likely that any essay, however learned or accurate it may be, will be capable of shaking the faith of any staunch supporter of a favourite creed. No Wernerian will give up his *formations*, his *chemical* and *mechanical* deposits, his alternate *retreats* and *returns* of the original ocean in which all things were dissolved, nor the gradual diminution of the level of the different rocks according to the order of their deposition. The faith of no volcanist will be shaken in the volcanic nature of basalt, greenstone, and porphyry-slate, and in the identity of these rocks

with lava emitted from a burning mountain. No Huttonian will give up the notion that this globe had no beginning, and will have no end; that the level of the mountains is gradually sinking; that the rivers, and the rain, and the weather, are gradually washing them down, and carrying them into the sea; that they will be again hardened and elevated by a subterranean heat; and that granite, greenstone, and basalt, are the newest rocks upon the face of the earth. All the attempts made by contemporary writers to shake the faith of Deluc in the doctrine of the subsidence of strata, in consequence of the great cavities originally existing in the internal parts of the earth, were unavailing. We may safely predict that this will be always the case nine times out of ten whenever attempts are made to improve a science by altering the opinions generally received by the learned of the age. When Harvey demonstrated the circulation of the blood, his opinion, it was remarked, was not embraced by a single medical man beyond the age of 40. How many years elapsed before the Newtonian theory of gravitation, now so firmly established, made its way even into the Academy of Sciences, a body of men who have always boasted not a little of their liberality! Mr. Cavendish continued an advocate for the existence of phlogiston to the end of his life; at least he never publicly admitted the contrary doctrine, and chose rather to relinquish his chemical investigations than to acknowledge that the opinions which he had supported were erroneous. Were it not that it would have an invidious appearance, it would be easy to point out some very recent and striking instances of the same line of conduct.

These examples are more than sufficient to show that we have but little chance of altering the opinions of those who have been long devoted to the study of any particular science, and that the effect of new books and new views is almost wholly confined to those who are only starting into the arena of science, or at least have not set themselves up as masters and teachers. There is a pride of consistency which has a greater effect upon the minds of most men than they are willing to acknowledge, and which is generally sufficient to prevent those who have openly advocated an opinion, or set of opinions, from afterwards giving them up. Books of science, therefore, ought always to be written in such a way that they may be understood by readers though they are not so much conversant with the subject as the writers themselves. This I consider as a mistake into which our author has inadvertently fallen. He seems to take it for granted that his readers are acquainted with all the subjects of discussion which have occupied the attention of geologists, with the various sects into which the proficient in this fascinating science have been divided, and with the different views which each leader, or would be leader, of a sect has advanced. This has prevented that rigid attention to arrangement which is always of so much

advantage to the author himself. The Essay seems to have been written at different times, and with different objects in view, and the subjects are merely placed side by side, or rather one after the other, without sufficient attention to render the bond of connexion evident to the reader. Hence a certain air of obscurity in which the opinions of the author are involved; and I am not sure if he has been always able to avoid inconsistencies and inaccuracies in his reasonings; but these, if they happen to exist, will be best pointed out in the analysis of the Essay to which I now proceed.

The figure of the earth which it actually presents, is called its *real* figure. Its *statical* figure is that which it would present if level with the surface of the sea.

It is well known that the statical figure is an oblate spheroid, and that mathematicians have demonstrated that this is the figure which it would have assumed had it been in a liquid state. The Huttonians, or at least Prof. Playfair, are of opinion that the earth had no beginning, and, therefore, that it never was liquid, but that it owes its statical figure to attrition and volcanic fire. It appears to me that the inaccuracy of this opinion is capable of being shown by mathematical demonstration.

The real figure of the earth, is known to be uneven on the surface; in some places rising into mountains; in others sinking into valleys. These inequalities, when compared with the size of the globe, sink into absolute insignificance; but with regard to man, they are of the highest importance, as his comforts, or probably his very existence, depends upon them; for if there were no mountains, there would be no rain, and no rivers; and consequently the earth would be incapable of producing food for the support of its inhabitants. Much discussion has taken place respecting the cause of these inequalities. Our author is of opinion that the valleys have been produced by the removal of matter. It seems to follow as a consequence from this, though our author nowhere states it, and indeed in the third essay expressly embraces the contrary doctrine—it seems to follow, I think, that before the valleys were excavated, the surface of the earth was level, or nearly so; which was the doctrine of Burnet.

That valleys have been usually formed by the removal of matter, our author considers as established by the following considerations: When strata are horizontal, it is usual to find opposite hills of the same elevation composed of the same substances. Of this, various examples are given; but the fact is so common that it cannot have escaped the notice of the most careless observer. The strata found on the heights are wanting in the valleys. But if we follow the beds which occur in the valleys, we find them running under the hills, and constituting a part of their substance. It follows from this, that the valleys did not exist at first, but the strata continued without interruption till



they were removed from those parts at present constituting the valleys.

Another evidence that this has been the origin of valleys is, that when a vein or a dyke passes through a rock, it is not cut off by a valley, but appears again in the rock on the other side of the valley: indicating that at first it passed without interruption through the strata which formerly filled up the valley before, it was excavated.

We can very frequently observe the same coincidence in the nature of the soil, gravel, and boulderstones, on the opposite sides of a valley.

It is true that the coincidences here pointed out are not always to be observed, and Deluc made use of this want of universal coincidence as an objection to the doctrine that valleys owe their existence to excavation. But our author considers Deluc's objection as of no force, because valleys often run along the line of the junction of two different rocks, and in such cases a coincidence cannot be looked for.

Our author further observes, in corroboration of his opinion, that we often observe a correspondence in the position of strata on the opposite sides of valleys. This correspondence in dip does not indeed prove that strata have been united, provided we find a different dip in the intervening space; but when two opposite cliffs correspond in every respect, we can entertain no reasonable doubts that they once were united.

Boulderstones, so abundant on the north side of the Alps, and on the south side of the Baltic, further evince the excavation of valleys. They may be often traced to the place whence they came. Of this, our author gives many examples. Thus the blocks of granite in Cheshire, Staffordshire, Shropshire, &c. may be traced to the Cambrian mountains. I may add another example which I believe to be rather uncommon. The merse, or low part, of Berwickshire, contains many boulders of a greenstone distinguished by large crystals of greenish-white felspar. These boulders may be traced to the cheviots which lie at a considerable distance to the south-west.

Another proof that valleys have been excavated, and that they have been excavated by water, is, that very frequently the opposite projections and indentations of the bounding mountains correspond. This fact was first noticed by Bourguet in 1729; but did not attract much attention till illustrated by the eloquence of Buffon. Humboldt, when he says that this agreement may be observed on the opposite coasts of the Atlantic, in the opposite projections and indentations of the old and new continents, has extended the doctrine too far. We can conceive no flow of water of such magnitude as to be capable of excavating the Atlantic and indenting the sides of the opposite continents. Such generalizations are too vague to be of any utility, and can serve no other purpose but to mislead.

Valleys usually widen as they advance; but this is modified by two disturbing causes. 1. The breadth of valleys depends on the comparative hardness of the substances which bound them. Hence where the bounding substances become harder, the valley may contract in its dimensions as it advances. 2. The size and direction of a valley change as often as it is joined by lateral valleys.

From the phenomena of valleys, it is obvious that they have been formed by running water, and consequently mountains do not owe their origin to volcanoes.

It is a very common thing to observe a series of valleys including each other. Thus the valley of the Thames at London is contained in that of which Clapham Rise forms part of the boundary on one side, and the Green Park on the other, and this again is contained in the large valley between Highgate and Sydenham. Arrived at these points, we find our horizon bounded by a chalk ridge still loftier.

Another phenomenon from which we derive the knowledge that a debacle, or flood, has at one time or other swept the present surface of the earth, is the vast quantity of alluvial matter scattered in such profusion over immense tracts of country. These matters have been derived from the breaking up of rocks situated on a higher level than themselves. The largest masses are found nearest the parent rock, and they diminish in size according to their distance. The blocks and pebbles found at the greatest distance are composed of the hardest and most indestructible materials. Substances breaking into cubic or hexagonal blocks are found at a greater distance than those which break into fragments with acute angles. Hence the reason why granite is found at such a distance from the parent rock.

It is obvious that the blocks of granite found on the surface of alluvial tracts are too large to have been transported by ordinary rivers; neither can we ascribe the excavation of valleys to the action of rivers; for,

1. Some valleys are dry, and could not possibly have been excavated by a river which does not exist.

2. The source of the river is often below the head of the valley; demonstrating that the valley existed before the river.

3. The alluvial land often reaches far below the bed of the river. In such cases it is evident that the river did not excavate the valley; on the contrary, it has been employed in filling up the valley.

4. Both exterior and interior valleys cannot be ascribed to the action of the same river.

5. Rivers change their beds, and therefore are not fitted for excavations.

6. Rivers cannot be supposed to have formed their banks without having been at one time without them—a supposition which is absurd.



Such are the arguments brought forward by our author to show that valleys were not excavated by rivers. Some of them possess force; but others, the last two, for example, seem to me to be of little weight. Perhaps, therefore, it would have been better to have omitted them. Our author has not paid much attention to the rhetorical, or even the logical method of arrangement, nor to a maxim of which reasoning authors should never lose sight, that whatever does not strengthen their reasoning in reality weakens it.

Buffon ascribed the excavation of valleys to the ordinary action of the sea at a time when it covered the present surface of the earth. But our author is of opinion that this cause is inadequate, and that they must have been excavated by a debacle, or deluge, which must have been universal, or at least exceeded the height of the highest mountain.

Pallas ascribed this debacle to the tremendous volcanic eruptions, which, in his opinion, occasioned the Moluccas, Philippines, &c. to emerge from the sea. His reason for adopting this notion was, that he thought that it afforded an explanation of the existence of the bones of tropical animals in Siberia. But our author thinks the explanation insufficient, for the following reasons:

1. Had the carcasses of tropical animals been wafted by a debacle, moving with violence sufficient to excavate valleys, and waft along huge boulderstones, they must have been abraded and dashed to pieces before they reached the polar regions of Siberia; yet they have been found entire, and even covered with the skin and hair.

2. Were we to suppose them to have moved only at the rate of 100 miles a day, they must have been putrid long before they reached their destination, which our author estimates at 36,000 miles. I am at a loss to conceive the data upon which this calculation is founded. Were we to reckon a degree of mean latitude 70 miles, which is considerably above the truth, the distance between the equator and the pole would only amount to  $70 \times 90 = 6,300$  miles, which is not much more than a sixth of the distance stated by Mr. Greenough. The length of a meridional circle, surrounding the globe of the earth, and passing through both poles, does not much exceed 25,000 miles, or two-thirds of the distance which our author conceives to exist between the equator and the pole.

3. With the bones of the elephant and rhinoceros are intermixed those of the elk, gazelle, horse, ox, buffalo, animals which inhabit northern climates.

4. Granting the debacle to have been occasioned by the sudden elevation of the Moluccas, &c. no reason can be assigned why it took a northern rather than a southern direction.

5. The rising of these islands would have been inadequate to have produced the effect assigned by Pallas.

Of these arguments, the only one which appears to me to possess weight is the last. Before we can be in a capacity to judge whether Pallas's cause be inadequate or not, it would be requisite to know the size of the islands alleged to have started out of the ocean, and the depth of the ocean in that part before their appearance.

Our author is as little disposed to admit the explanation of the origin of the debacle given by Sir James Hall; namely, the elevation of our present continents by explosions. Such an elevation would doubtless have thrown the ocean into a violent agitation; but how this agitation could have excavated the valleys, and covered with alluvial deposits the very continents which were elevated above its action, is a question which does not admit of an easy solution.

Our author is of opinion that the universal diffusion of alluvial sand, gravel, &c. demonstrates that a universal inundation has formerly taken place. That these alluvial deposits abound in low-lying countries is a fact that admits of no dispute; but I am not aware of any evidence of the existence of these substances at great heights above the level of the sea. If any confidence can be placed in Humboldt's observations, no mountain exists in America above the height of 10,000 feet which is not volcanic. The Alps contain no alluvial deposits at great heights. The same thing may be said of every range of primitive mountains hitherto examined; unless we were to consider small-grained granite and granular quartz as alluvial deposits. These indeed are so connected with the other rocks, that if their alluvial nature were admitted, that of almost all other rocks would follow as a consequence. But as our author does not consider the calcareous and other rocks which abound in fossils to have been formed by the debacle, but to have existed before that period; neither can he admit the small-grained granite, granular quartz, &c. to have been owing to his debacle. Of course his argument for the universal distribution of alluvial debris remains unsupported.

The universality of the deluge, he thinks likewise demonstrated by the universal occurrence of mountains and valleys. And certainly if it be admitted that valleys were excavated by the deluge, this argument is conclusive.

The figure of the surface hardly ever corresponds with the disposition of the strata and veins beneath it. This shows that the figure of the surface has been altered; but whether by a debacle, or by the gradual action of the weather, &c. is not so clear.

Our author enters into a disquisition respecting the structure of the surface of the globe. He thinks that towards the equator there is a belt of mountain that encircles the earth; while both to the north and south, these high grounds are bounded by low

or sandy plains. I think it unnecessary to follow him in this disquisition, upon which he lays little stress, and which appears rather fanciful than real.

He agrees with Deluc, Dolomieu, and Cuvier, in thinking that not above 5000 or 6000 years have elapsed since the period of the deluge.

Before the deluge, he is of opinion that the order of things was nearly the same as at present; for the following reasons:

1. The earth must have moved round its axis. Hence the sun and planets must have existed as they do at present.

2. In the diluvian detritus, occur bones of the horse, ox, stag, &c. Hence these animals must have existed before the deluge. Of course the atmosphere, the climate, and the face of the globe, must have been nearly the same as at present.

3. The other planets are spheroidal, like the earth, and, therefore, must have had a similar origin.

The earth must have been divided into land and water before the deluge just as at present; for the remains of sea and land productions are universally diffused through the secondary rocks; but the situation of sea and land was probably different from what it is at present. Our author is of opinion that the antediluvian earth is now covered with sea, and that our continents before the deluge were immersed under the ocean. In this notion, he agrees with Deluc and with the Huttonians. The occurrence of boulderstones, &c. proves, he conceives, the non-existence of the Mediterranean, German, and Baltic Seas, at the time that these bowlders were deposited. I must acknowledge myself unable to perceive the force of this reasoning.

The newest formations are intersected by valleys, and covered by alluvial deposits. Hence the deluge was posterior to the birth of these formations. Of course it was posterior to the interment of the fossil organic bodies which these rocks contain. Hence these fossils belonged to a former world. The deluge was posterior to the formation of mineral veins. We have no positive evidence whether the deluge happened before or after the birth of man.

We are unable to explain the way in which the deluge happened. Our author does not think that it can be ascribed to the increase of water, or to the subsidence of continents. The water must have been in violent agitation. Probably the quantity of water at present existing, if violently agitated, would have been sufficient to produce a deluge. He is of opinion that the cause was external, and seems inclined to ascribe it to the blow of a comet. Were we to enter into any speculations respecting so wild and hopeless a subject, a much simpler cause might be assigned; namely, the alteration of the axis of the earth from the equator to the poles. This would have covered all the old continents, and set new continents above the level of the sea.

It would have produced an enormous agitation, and would explain the seeming alteration which has taken place in the northern climates.

Such is our author's reasoning on the debacle or deluge. It possesses considerable plausibility as he has placed the arguments. But I have no doubt that Deluc, were he now alive, and in the vigour of his understanding, could write an equally plausible refutation of the whole essay; that Sir James Hall could give an air of plausibility to his doctrine of elevations; and that Mr. Jameson could state very plausible reasons for supposing that the mountains and valleys either existed originally, or have been produced by the gradual action of the weather. The subject does not admit of precise reasoning. I consider it as but of second-rate importance, and am of opinion that those persons who confine the science of geology to such speculations mistake its true nature, and rather injure than promote its progress by calling off the attention of its cultivators from the investigation of facts to loose discussions which are not susceptible of accurate demonstration.

(To be continued.)

## ARTICLE X.

### *Proceedings of Philosophical Societies.*

#### ROYAL ACADEMY OF SCIENCES AT PARIS.

#### *An Analysis of the Labours of the Royal Academy of Sciences during the Year 1818.*

(Continued from p. 225.)

*Memoirs read to the Academy, but which have not as yet been transmitted to us.*

On the Motion of Elastic Fluids in Cylindrical Tubes; by M. Poisson.

A Notice on bringing the Colorigrade to Perfection; by M. Biot.

On the Usefulness of the Laws of the Polarization of Light, in finding the State of Crystallization, and of Combination in those Cases where the Crystalline System cannot be observed in a direct Manner; by M. Biot.

#### PRINTED WORKS.

*Exercises on the Integral Calculus; Construction of Elliptic Tables;* continuation of vol. iii.—M. Legendre continues his vast and useful labours. The determination of the functions E and F, according to the different values of the



amplitude, and module is the object which he investigates in this continuation of his researches. "This may be obtained, either by means of a table constructed for each given value of the angle of the module, or by means of a system of tables, which shall be constructed according to the variation, by equal and sufficiently small intervals of the amplitude and the angle of the module." The author discusses the advantages and difficulties of each of these methods. The second supposes a task, which it would take a long time to execute. To lessen in some measure the difficulty, his tables 8 and 9 offer a preparatory labour to calculators, which may also partly supply the want of more extensive tables; but as they proceed only from one degree to another, either of the amplitude or of the angle of the module, their interpolation will necessarily be more difficult, or less exact than if these intervals were smaller.

In order to avoid double interpolations, it would be necessary to return to the first method; but the calculation of this table would be so long that there ought to be a great number of functions to calculate upon the same module, to induce one to undertake so considerable a preliminary work: to attain the same end with greater ease, the author shows that a table of only a few lines, formed upon a given module, will serve to calculate, as far as 10 decimal places, or more, the functions E and F correspondent to any value of the amplitude  $\phi$ ; and that it will be sufficient for this purpose to add to the common mode of calculating the interpolation, that of some very easy trigonometrical formulæ. This method may be rendered still more simple, if the calculation is confined to seven decimal places; but it is explained in detail, and applied to examples, with the utmost care, in order that the exactness of the results may be warranted as far as the fourteenth decimal place. This precision may probably never be required: it is the utmost limit of exactness which can be obtained by the tables now known. The tables of the logarithms of numbers, by means of some artifice in calculation, would show it to the 20th or 22d decimal places; but beyond that number, common arithmetical calculations must be resorted to, by which alone, an indefinite degree of exactness may be obtained.

Such is in substance the introduction placed by the author at the beginning of a work, of which it is impossible for us to give a neater or more complete idea. The reader will find it rich in formulæ, in acute developments, and in subsidiary tables scrupulously calculated, some to 10, and others to 14 places of decimals, with differences as far as the third or fourth order.

*History of the Astronomy of the Middle Ages*, pp. 700, 4to. with 17 plates, by M. Delambre.—The author calls the middle age of astronomy the interval between the period when the Greeks ceasing to write had their place supplied by the Arabians, Persians, and Tartars, and that, when Copernicus, restoring to

the earth the motion which had been falsely attributed to the sun, merited the appellation of being the founder of modern astronomy.

The Arabians preserved religiously the theories of the Greeks, and made no change either in the form of their instruments, or in the manner of using them; but they had them much larger and better divided; the number of their observers was also much greater; and from the time of Almamoun, very apparent improvements in respect to the elements of the theory of the sun, the obliquity of the ecliptic, and the precession of the equinoxes, are to be observed in their works. The introduction of sines and versed sines by Albategnius; of tangents by Aboul-Wesa; of subsidiary arcs to simplify a complex formula by Ebn-Jounis, entirely changed the manner of making astronomical calculations. The Arabians also marked the times of phenomena with more precision than the Greeks. Albategnius deduced from the analemma a rule, in two parts, to find the altitude of a star by the altitude of the pole, the declination, and the horary angle: this is now the fundamental formula of modern trigonometry. The same author then altered this formula by substituting the versed sine for the cosine in order to find the hour by the altitude of a star whose declination and right ascension are known. It appears, however, that the Arabs made very little use of this second rule, and that most commonly they found it sufficient to determine the hour, after the manner of Hipparchus, by means of an astrolabe, or planisphere, which served at the same time to observe the altitude, and to dispense with a trigonometrical calculation. It was shown in the *Ancient Astronomy* that Hipparchus invented this instrument to find the hour of the night by means of a star; of course he could find the hour of the day much easier by the sun.

The Arabians, in adopting the Greek astronomy, were not less careful in collecting the astrological reveries of the Chaldeans: they applied their trigonometry to them, and invented new methods of dividing the heavens into 12 houses. These methods, brought to perfection by Regiomontanus, and especially by Magini, are translated into modern formulæ, and the whole of them applied to the calculation of the same nativity; so that a judgment may be formed of their differences, and of the uncertainties which they must add to those predictions, of which the fundamental principles are, in other respects, a mass of purely arbitrary suppositions, the fruits of credulity, or rather of charlatanism.

Dialling, which is at present considered only as a curious application of astronomy, constituted at that time an integral part of it, from the commodiousness of good sun-dials in exhibiting the civil hours, and in regulating clepsydras. Without changing in the least the theory of the Greeks, the Arabs, studying the analemma of Ptolemy, found means to derive from



it easier and far more varied solutions; and they invented a number of dials, both fixed and portable. Aboul-Hassan deduced from the conic sections very curious rules for tracing the arcs of the signs independently of the hour lines. Nevertheless his method had neither the simplicity nor the generalisation which it was possible to give to his needlessly complicated problems. He did not see that in every dial that can be described on a plane, the parameter of the section is always the same, being equal to twice the co-tangent of the declination of the sun, and that this depended, neither on the section of the cone, nor on the height of the pole upon the plane. No one as yet had perceived this theorem; no one had dreamed of disposing the general equation of the conic sections in a manner peculiarly adapted to dialling. This equation was at length published, depending only on the height of the pole, the declination of the sun, and lastly on the height of the gnomon, which it is convenient to assume as unity. There results from this formula an extremely simple mechanical method of describing the arcs of the signs, the trigonometrical calculation of which is always very long, and also requires the horary angle of the plane. Aboul-Hassan first made the curious remark that every plane may be considered as the horizon of a place, whose geographical longitude and latitude might be easily determined: he first gave a projection of the pole on the plane, and consequently a point common to all the equinoctial hour lines: he also first spoke of substituting these equal hours in place of the ancient unequal hours, of which the Arabs then made use in imitation of the Chaldeans and Greeks.

Dialling, on passing into Europe, underwent other changes; instead of the straight style, an axis was substituted, the entire shadow of which covered successively all the hour lines; instead of the arcs of the signs, mechanical constructions were invented, which, being reduced to formulæ, are found to be identically the same as the modern methods. But the Europeans demonstrated nothing, neither did the Arabians: their works are frequently unintelligible; and in order to demonstrate their obscure practices, their historian has found it necessary to include in about 60 trigonometrical formulæ every thing that concerns this part of astronomical knowledge; he finds among them the demonstrations which Clavius was not able to discover, and also draws from them new methods of tracing, either mechanically, or by calculation, the arcs of the signs, the meridian line of mean time, and all the hour lines of dials without any centre, whether Babylonian, Italian, or French.

The Persians and Tartars showed the same respect to the Greek theory; they adopted the Arabian trigonometry; they improved our knowledge of the motion of the sun; and we owe to the latter a new catalogue of the stars; namely, that of Ulugh-beigh. The first European astronomers were equally servile

imitators. Alphonso caused new tables to be calculated, which were employed for 200 years. Regiomontanus declared several times that they were erroneous ; but he had neither time nor the means to compose better. He engaged much in trigonometry, but did not proceed so far as Aboul-Wesa, or Ebn-Jounis ; he did not perceive the usefulness of tangents, the formulæ for which he found in Albategnius. Our trigonometrical system was completed by Vieta, who published a table in which the tangents and secants were for the first time united with the sines, and who afterwards published the four analytical formulæ which are sufficient for the solution of every case of oblique-angled spherical triangles. To him we owe some curious and very useful formulæ for tangents, secants, and even for sines ; he created that theory which he called angular sections ; lastly, from his theorems, although frequently very obscure, the expression of the first and second differences of sines may be drawn, and more expeditious and surer methods for the construction of trigonometrical tables than those he has himself mentioned.

We have only been able to indicate, in a very brief manner, the subjects that are treated in this astronomy of the middle ages, which also includes an analysis of all the works that are any ways remarkable, which appeared in this interval of more than 600 years, ending with the appearance of Copernicus. This restorer of astronomy, with Tycho, Kepler, and Galileo, will furnish abundant matter for the first volume of Modern Astronomy, which is in the press.

We mentioned in the History of 1817, the obligations we were under to M. Sedillot for what concerns the Arabs and the Tartars.

*Notice on the Operations which are undertaken to determine the Figure of the Earth ;* by M. Biot.—This hasty sketch of every thing that has been done for this 150 years, in regard to determining the figure of the earth, was listened to with great interest in the public meeting of the four Academies. The description of the Shetland islands, so new to us, where the author charged with these delicate and difficult operations found the most cordial hospitality and every attention that could ensure the success of his enterprise, was particularly applauded. The whole of this recital will be found in our Memoirs. We confine ourselves at present to that which constitutes the object of the voyage, and announce with pleasure that M. Biot has brought home with him 38 series of observations on the pendulum, of five or six hours each ; 1,400 observations of latitude in 55 series, taken as many to the north as to the south of the zenith, and about 1200 heights of the sun, to determine the going of the clock. Such part of the calculations as time has permitted to be executed proves that the results will be conformable to those already deduced from the theory of the

moon, and from the measurement of terrestrial degrees compared together at great distances.

*Memoir upon the subterraneous Inundations, to which many Quarters of Paris are periodically exposed*; by M. Girard.—A subterraneous source of water has manifested itself for some months in the northern quarters of Paris, which has produced an inundation of a great number of cellars, and has spread itself on the surface of some marshes situated below the hospital of St. Louis, between the faubourg of the Temple and the faubourg St. Martin.

Buache, of the Academy of Sciences, and Bonami, of the Academy of Inscriptions, had already endeavoured to discover the probable cause of similar inundations which were observed in 1740, and renewed in 1788. Perronet was consulted, and in a report made by him to the Prevôt des Marchands, he pointed out the true cause. M. Girard exhibits this cause in a new light, and supports his explanation by facts and reasoning; so that it admits no longer of any doubt. In 1788, some people attributed these inundations to the reservoir of the steam-engine just established at Chaillot. Lately they accused the basin of la Villette. M. Girard shows with great ease that such causes as constantly exist cannot serve to explain inundations which arise only after long intervals. In adopting the idea of Perronet, he ascends to the origin of the cause, explains why these inundations did not happen before that time, and also shows by what concurrence of circumstances they have been observed to take place three times since 1740.

Formerly a rivulet which runs from Menil-montant toward Chaillot received the water that drained from Paris and the neighbouring hills. This drain was embanked with walls, and at last arched over. The ground-plot of Paris enlarging, new streets arose upon ground made of rubbish. These buildings prevent the running off of the waters, which, soaking gradually into the earth, raise by degrees the level of the subterranean waters: in very wet years, these waters spread themselves into cellars, which otherwise, by their distance from the rivulet, would appear to be in no danger of such an accident. On consulting the registers of meteorological observations, M. Girard found that in 1786 and 1787 there fell 124 centimetres of rain, and that the rainy days were 324; so that in the space of these two years the quantity of rain that fell was one-fifth more than in an average year, and the rainy days were also one-seventh more than usual: hence arose the inundation of 1788. The same circumstances took place in 1816 and 1817, and occasioned the inundation of 1818.

M. Girard, in concluding his memoir, observes, "If it be recollected that these subterranean inundations have hitherto only taken place at intervals of 30 years, and that they have been

occasioned by circumstances absolutely similar in themselves, it may be concluded that this accident will seldom occur; for it depends not only on the excessive quantity of rain, but also on its continuance. This much is certain, that on account of the obstacles which have been at different times put in the way to prevent the free drainage of the rain water in the northern quarters of Paris, every time the depth of rain that falls in the space of two years shall be greater than 120 centimetres, and the number of rainy days in that interval shall be more than 320, those quarters of Paris which are situated on the right bank of the Seine will have reason to expect a subterraneous inundation the next year."

*Memoir on the Topography and Levelling of Paris*; by M. Girard.—The three islands formed by the Seine, and the quarters extending to the north and south, were formerly meadows, which were overflowed by the Seine every time that there happened a flood. The gravel carried by the stream, and the mud which was suspended in the water, were deposited on the surface of the meadows. Every year a new layer of these deposits raised the soil, while at the same time similar depositions also elevated the bed of the river. As the valley became peopled, the necessity of preventing these inundations obliged the inhabitants to accelerate the operations of nature by bringing fresh earth, or to raise dikes or quays upon the shores of the river, which might prevent these overflowings; and the bed of the Seine being continually elevated, the quays were forced to be frequently raised; and the surface of different quarters heightened artificially. The rubbish carried out of the inhabited part formed in time the rising grounds of Moulins, and of Notre Dame de bonne Nouvelle, the small hills of rue Hyacinthe, rue Taranne, and the labyrinth of the Jardin des Plantes. The population increasing, the surfaces of these deposits of rubbish were partially levelled, and new streets traced upon them. The Boulevards towards the north, being the remains of the ancient ramparts, form a spot of ground which is higher than the rest of the city and neighbouring fauxbourgs.

It has been long proposed to determine the real level of the different quarters. In 1742, Buache formed the project of performing a hydrographical plan of Paris. The inundation of 1740 furnished the original data of a general level of the quarters that had been laid under water. He afterwards drew several profiles of the ground, by traversing the town in different directions. This work is the only one hitherto published. In order to begin a new work upon a larger scale, there were traced upon Vernique's large plan the heights of different points of the surface, in reference to a horizontal plane raised about 76 metres above the lowest level of the Seine. After having carefully verified these primary positions, successive additions were made, and



the levelled points brought nearer to each other. The level of the intersections of every street was thus obtained, and afterwards that of the intermediate spaces. The points which were found to be on the same level were then joined by straight lines, which produced a number of irregular polygons, whose traces showed the intersection of the surface with several horizontal planes, each of which was raised a metre above the other; so that these curves show the limits of the grounds that would be successively inundated if the water of the Seine were to rise metre by metre above its ordinary level.

*Memoirs on the Navy, and on the Civil Enginery of France and England containing an Account of two Journeys made by the Author to the Sea-Ports of England, Scotland, and Ireland; by M. C. Dupin, of the Academy of Sciences.*—This collection is much more interesting than its title would lead one to suppose. The account of the two journeys contains only the historical and descriptive part; and it makes one wish for a more extensive work, which indeed the author promises, and which will also contain the theoretical part. This account is succeeded by a notice relative to the construction of the breakwater at Plymouth, and a description of the Caledonian canal, interesting to every class of readers; a plan of a large work already in great forwardness on the naval architecture of the 16th and 19th centuries; experiments on the flexibility and elasticity of wood; a very extensive memoir on the re-establishment of the Naval Academy, with much useful advice; another memoir on the actual state of the maritime museum established in the arsenal of Toulon, and on the works of Puget which are preserved in that arsenal; and lastly, a description of the machines for the naval service constructed at Rochfort from the plans of M. Hubert. The whole is accompanied with reports which have been made to the Academy of Sciences, the Academy of Arts, and by the naval officers at Toulon, when the author submitted his different works to their inspection. The History of the Institute will exhibit the reports which were read to the two Academies; and, therefore, we confine ourselves to the transcription of the conclusion of that made by the naval officers. “The commissioners think that the general plan of the author is well adapted to produce a good, useful, and interesting work, worthy of the encouragement of a great nation (this refers to a view of naval architecture); and they hope the Minister for the Marine will not refuse to the young author noble and honourable encouragements, the principal of which have been just pointed out.” They said a little before this, “that it was desirable M. Dupin should be enabled to examine the principal sea-ports of France and foreign countries, in order to form an impartial judgment of what concerns them, to collect together the most important materials, and to put them in practice with that unshaken constancy which the

author shows in the pursuit of his useful enterprises." This wish of the commissioners was expressed in 1814, and the collection of papers now announced proves that he has realised it in a great measure, and with good success.

(To be continued.)

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## ARTICLE XI.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Chalk in Bulgaria.*

It appears from Dr. Macmichael's Journey from Moscow to Constantinople, just published, that chalk exists on the south side of the Danube, near Rudschuk. This is a fact which was new to me at least, and probably will be so also to most of my readers. I was not aware of the existence of chalk in any part of the Turkish dominions.

#### II. *Mount Hæmus.*

This celebrated mountain, at present known by the name of Balkan, divides Bulgaria from Romelia. According to the account given of it by Dr. Macmichael, the strata on the north side are generally calcareous, and the summit is a blue or variegated marble. The moment you begin to descend to the south, the nature of the rock suddenly changes into a hard argillaceous schist, abounding in large veins of quartz.—(Macmichael's Journey, p. 141.)

#### III. *Moscow.*

The neighbourhood of Moscow consists principally of an argillaceous bed, containing numerous iridescent ammonites, and other fossils, and resembling the London clay; of a bed of sand or sandstone, which probably corresponds with the Kentish rag, or green sand formation; and of an oolitic limestone approaching the character of Portland stone. Considerable blocks of granite are found loose on the surface of the soil.—(Ibid. p. 31.)

#### IV. *Bathing in the Dead Sea.*

It is well known that the water of this sea is saturated with salt, chiefly muriate of magnesia, and common salt. Its specific gravity is 1.211. Mr. Legh, who bathed in it in 1818, informs us, that he saw several shell-fish in it, not unlike periwinkles. The account which he gives of the effects of bathing is singular, but not very intelligible. I shall give it in his own words. "Our Arab guides had endeavoured to alarm us as to the consequences of bathing in these pestiferous waters; but we made the experi-



ment, and found that though two of our party were unable to swim, they were buoyed up in a most extraordinary manner. The sensation perceived immediately upon dipping was, that we had lost our sight; and any part of the body that happened to be excoriated smarted excessively. The taste of the water was bitter and intolerably saline. From this experiment some of us suffered a good deal of inconvenience, an oily incrustation being left upon the body, which no attempt at washing could remove for some time; and several of the party continued to lose portions of skin for many successive days."—(Ibid. p. 192.)

I can understand the meaning of every part of the preceding description, except the alleged loss of sight. It deserves notice that the specimens of salt collected from the southern extremity of the Dead Sea did not deliquesce. This was doubtless owing to the dryness of the climate; not to the purity of the salt.

#### V. *Climate of Moscow.*

The severest frost of the winter 1817—1818 was equal to  $-28^{\circ}$  Reaumur ( $-31^{\circ}$  Fahrenheit). In Petersburg they had  $-30^{\circ}$  ( $-35\frac{1}{2}^{\circ}$  Fahrenheit). But this winter was reckoned a mild one, for the quantity of snow was unusually great; the best proof of its mildness; for in very severe weather, there falls but little snow.—(Ibid. p. 271.)

#### VI. *Population of Moscow.*

When the French quitted Moscow, there were only 16,000 inhabitants; but in the winter of 1817—1818, the population amounted to 312,000, including 21,000 military.—(Ibid.)

#### VII. *New Acetate of Lead.*

Charles Macintosh, Esq. who has long been a manufacturer of sugar of lead in Glasgow, accidentally obtained, some months ago, a salt in large flat rhomboidal crystals, which obviously differed in their shape from the crystals of common sugar of lead. Happening to visit the manufactory while these crystals were still in Mr. Macintosh's possession, I carried away a sufficient quantity of them to enable me to determine their properties and composition.

The crystals were white and translucent, and consisted of flat rhomboidal prisms, with angles of  $106^{\circ}$  and  $74^{\circ}$ . Each prism was terminated by a dihedral summit, formed by two faces proceeding from the narrow faces of the prism, and meeting at an angle of  $130^{\circ}$ . These crystals were not altered by exposure to the air.

The taste of the salt was sweet and astringent, similar to the taste of common sugar of lead. Its specific gravity was 2.575. At the temperature of  $60^{\circ}$ , 100 parts of water dissolved 34.8 parts of the salt. It was soluble also in alcohol, as is the case with common sugar of lead.

When heated, it melted and boiled, giving out water in the first place; but as the heat increased, the smell of acetic acid became sensible. I kept a quantity of the salt for a few minutes in the temperature of  $504^{\circ}$ ; it boiled violently for a little; but the boiling stopped, and it became solid almost at once. It was then an orange-coloured powder consisting entirely of a mixture of two parts protoxide of lead and one part metallic lead.

To determine the quantity of acetic acid in this salt, I dissolved 50 gr. of it in distilled water, and precipitated the lead by means of bicarbonate of potash, noting carefully the weight of bicarbonate necessary just to precipitate the whole of the oxide of lead. Two different experiments indicated the quantity of acetic acid in 50 gr. of the salt to be 11 gr.; so that 100 gr. of the salt contain 22 gr. of acid. The theoretical quantity, as will be seen below, is 21.85. I ascribe the small excess indicated by my trials (which amounted to 0.15 gr.) to the difficulty of determining the exact point of saturation.

To determine the quantity of oxide of lead, I made three experiments. 1. I weighed the carbonate of lead procured by decomposing 50 gr. of the salt by bicarbonate of potash. 2. I exposed 50 gr. of the salt to a heat of  $504^{\circ}$ , and weighed the residue. I then dissolved off the oxide of lead by means of acetic acid, and weighed the residual metallic lead. 3. I precipitated 50 gr. of the salt by means of sulphuric acid, and weighed the sulphate of lead, after being well washed and dried. By these three methods I obtained nearly the same results. The mean quantity of oxide of lead was 29.5 gr.; so that 100 gr. of the salt contain 59 gr. of protoxide of lead. The theoretic quantity, as we shall see immediately, is 60 gr.; but as the products of each of the three experiments were collected on a filter, dried in the open air, then taken off the filter, and heated to redness in a platinum crucible, practical chemists will see that there must have been some loss sustained. Though the experiments were made carefully, yet I do not think it at all unlikely that a loss of half a grain might have been incurred, which is all that is requisite to make the result of experiment agree with the theoretic number.

If we suppose this salt to be composed of an atom of acetic acid and  $1\frac{1}{2}$  atom of oxide of lead, then its composition will be as follows:

Acetic acid .....	21.85
Protoxide of lead .....	60.00
Water .....	18.15
	<hr/>
	100.00

Now the quantities actually found by the preceding analysis were the following:

Acetic acid. . . . .	22
Protoxide of lead . . . . .	59
Water . . . . .	19
	<hr/>
	100

I consider the agreement as sufficiently near to leave no doubt respecting the constitution of the salt ; so that it is composed of 5 atoms oxide of lead, 4 atoms acetic acid, and 19 atoms water.

#### VIII. Remarkable Double Rainbow. By Mr. Macome.

DEAR SIR, (To Dr. Thomson.)

Paisley, Sept. 7, 1819.

The afternoon of the 5th current was distinguished in this neighbourhood by frequent and very brilliant exhibitions of the rainbow. One of these, which happened about five o'clock, attracted my particular notice from an appearance which it displayed, which to me was uncommon. The lower half of the spectrum, commencing with the green rays, was distinctly repeated in the regular and usual succession of the colours. There was no interval of space between the usual rainbow and this novel accession to it ; nor did the breadth of the spectrum with this addition appear to me to be much increased beyond its usual size. A dense cloud, or rather a descending shower, soon interposed, and took from my view this curious phenomenon. The secondary bow, with its inverted colours, was visible along with the primary. The arch of either did not extend far beyond the zenith, and it was the southern limb of the primary that exhibited the appearance above stated. It would gratify me much to inform me of any writer who has taken notice of any similar appearance. Excuse this trouble from, dear Sir,

Your obedient servant,

A. MACOME.

#### IX. Octahedral Iron Ore.

M. Robiquet made use of octahedral crystals of oxide of iron extracted from a steatitic rock of Corsica, in the formation of peroxide of iron ; but the colour of the oxide which he obtained was bad, resembling that of rust. This led him to suspect the presence of titanium in it. He dissolved a quantity of the ore in muriatic acid, and evaporated the solution to dryness. On redissolving the salt in water, a white insoluble powder remained, which, when heated to redness with a sufficient quantity of carbonate of potash, became soluble in acids, and exhibited all the characters of oxide of titanium. He extracted about six per cent. of this oxide from the octahedral ore which he examined. —(Journal de Pharmacie, 1819, June, p. 265.)

X. *Juice of Carrots as a Remedy in Cancer.*

I know not whether it be proper to mention an application in cases of cancer which Bouillon Lagrange has brought into notice, and which he assures us he has often employed with the happiest effects. The remedy is the juice of carrots applied externally, together with internal medicines suited to the circumstances of the patient. It is possible that carrot juice, or rather carrot poultice, applied in cases of cancer, may have the effect of alleviating the pain, which may render it of some value to medical practitioners; but to trust to such applications in cases of cancerous mammæ, as M. Bouillon Lagrange seems to advise, is a practice that we trust no surgeon in this country will venture upon. The following application, which the same practitioner recommends under the name of *anticancerous topic*, is little else than a modification of an old method often employed in this country and in Germany; but I never heard of any case where the application was attended with the cure of the disease.

R Simple plaster, ʒiv.

Melt it by a gentle heat in a stone-ware vessel, then add of

Yellow wax .....	ʒi
White soap .....	ʒss
Turpentine. ....	ʒiv

Melt this mixture, take it from the fire, and then add of

Leaves of hemlock powder ....	ʒij
Pounded sulphuret of potash ..	ʒij
Pounded camphor. ....	ʒiv

Mix exactly.—(See Journ. de Pharm. 1819, June, p. 255.)

XI. *Analysis of Galbanum.*

M. Meisner has obtained the following constituents from a set of experiments to determine the composition of *galbanum*:

Resin. ....	329
Gum. ....	113
Cerasin. ....	9
Malic acid. ....	1
Volatile oil. ....	17
Vegetable debris. ....	14
Loss. ....	17
	<hr/> 500

(Trommsdorf's Journal der Pharmacie, as quoted in Journal de Pharmacie, 1819, Juillet, p. 308.)

## XII. Action of Binormalate of Potash on Black Oxide of Manganese.

M. Van-Mons has observed, that if into any vessel containing black oxide of manganese we pour a hot solution of binormalate of potash, and after agitating the mixture throw it upon a filter, we obtain a liquid of a fine red colour. During the action of the acid on the oxide, a quantity of carbonic acid is disengaged. —(Journ. de Pharm. 1819, July, p. 307.)

## XIII. Meteorological Observations at Cork. By T. Holt, Esq. (With a Plate. See XCVIII.)

(To Dr. Thomson.)

SIR,

Cork, Aug. 10, 1819.

I ENCLOSE you the result of my meteorological observations made in and near Cork, for the second quarter of 1819; and am, Sir, with due respect,

Your obedient humble servant,

THOMAS HOLT.

### REMARKS.

#### APRIL.

1. Fine; some showers.
2. Fog; dry; cloudy.
- 3, 4, 5. Dry; cloudy.
6. Cloudy; breeze; showers.
7. Thick fog; showery.
8. Bright; cloudy evening.
- 9, 10. Bright.
- 11, 12. Bright; occasional showers.
13. Fair; rainy evening.
14. Bright.
15. Light showers.
16. Rainy morning; cloudy.
17. Ditto; rainy evening.
18. Bright; occasional hail showers.
19. Rainy morning; fine; rainy evening.
20. Rainy day.
21. Bright.
22. Ditto; some light showers.
23. Cloudy; misty evening.
24. Misty day; rainy night.
- 25, 26. Cloudy; dry.
27. Cloudy morning; rainy, p. m.
28. Rainy day; wind.
29. Dry; cloudy; rainy evening; gale.
30. Showery.

#### MAY.

1. Bright; rainy evening.
2. Rainy day.
3. Heavy showers.
- 4, 5. Bright days.
6. Rainy.
7. Cloudy; rainy evening.

8. Cloudy; some rain.
9. Bright.
10. Cloudy.
- 11, 12, 13, 14, 15, 16. Bright days.
- 17, 18. Light showers.
19. Dry; cloudy.
20. Showery.
21. Bright; rainy evening.
22. Showery.
23. Dry; cloudy; gale.
24. Showery.
25. Ditto; rainy evening.
26. Bright.
27. Frost last night; fine day.
- 28, 29. Bright days.
- 30, 31. Showery; rainy night.

#### JUNE.

- 1, 2. Bright; some showers.
3. Rainy morning; fine day.
- 4, 5. Bright; some showers.
6. Rainy.
7. Bright; occasional showers.
8. Fair; rain last night.
- 9, 10, 11. Bright; occasional showers.
12. Bright; dry.
- 13, 14. Ditto; showery.
- 15, 16, 17, 18, 19, 20, 21, 22. Bright, dry days.
23. Fine day; rainy evening.
24. Showery.
25. Rainy night and morning; fine day.
- 26, 27. Showery; rainy night.
- 28, 29, 30. Dry, cloudy days.



## RAIN.

1819.	Inches.	1819.	Inches.	1819.	Inches.
April 1	0·014	May 1	0·024	June 2	0·046
2	0·010	2	0·798	3	0·168
3	0·021	3	0·246	5	0·065
6	0·114	6	0·324	6	1·320
7	0·084	7	0·196	7	0·168
12	0·048	8	0·210	8	0·124
13	0·084	18	0·036	11	0·252
15	0·016	20	0·180	14	0·036
16	0·264	21	0·204	23	0·240
17	0·074	22	0·040	24	0·146
19	0·108	24	0·036	25	0·168
20	0·258	25	0·768	27	0·216
24	0·268	31	0·624		
27	0·124				2·949
28	0·245		3·686		3·686
29	0·268				2·241
30	0·241				
	2·241				8·876

XIV. *Method of preserving Water at Sea.*

M. Perinet, who has examined the different methods recommended for preventing water from becoming putrid at sea, considers the following as the best: He mixes black oxide of manganese with the water to be preserved in the proportion of  $1\frac{1}{4}$  of the former for every 250 parts of the water, and shakes this mixture once a fortnight. Black oxide of manganese has not merely the property of preventing water from becoming putrid; but M. Gay-Lussac informs us, that it sweetens water already putrid; but unfortunately the water retains a little of the oxide in solution (Ann. de Chim. et de Phys. xi. 110). I presume that this portion will be precipitated when the water is boiled. It is probably held in solution by sulphuretted hydrogen; unless indeed it has the property of converting sulphuretted hydrogen into sulphuric acid, which is not improbable, though it has not been determined by experiment.

XV. *New Method of preparing Pharmaceutical Extracts.*

By John T. Barry, Esq.

Mr. Barry's method consists in completing the evaporation of vegetable juices and infusions *in vacuo* by means of a simple apparatus which he has contrived for the purpose, and for which he has obtained a patent. By this mode of operating they are excluded from the action of the air, and at the same time the necessity of exposing them to a higher temperature than from  $90^{\circ}$  to  $100^{\circ}$  is superseded—two most important points in the preparation of this class of medicines. Extracts thus prepared differ from those in common use, not only in their physical, but their medicinal properties. Thus “the taste and smell of the extract of hem-

lock are remarkably different, as is the colour both of the soluble and feculent parts : it also possesses an extraordinary degree of tenacity, a property which is not generally found in the common extract ; and it abounds so much with crystallized matter as to produce a gritty sensation when rubbed between the fingers. The extract of belladonna contains a large quantity of crystals of some kind of salt ; but I have not," says Mr. Barry, "succeeded in procuring either of these in a separate state so as to give them an examination. In some attempts to obtain them pure, and more particularly to discover whether morphia was traceable in the extracts, I became acquainted with the singular fact, that phosphoric acid in a soluble state is to be found in all the extracts. On further extending the investigation, it was ascertained that this acid, besides that portion of it which exists as phosphate of lime, is contained in a vast variety of vegetables. It would be foreign to the subject of this paper to enumerate the substances that were tried ; but I may just mention that all those vegetables which are cultivated seem to contain phosphoric salt in great abundance.

"The extract of taraxacum is another remarkable instance of the difference in the sensible qualities of these preparations. Instead of being sweet to the taste, and high coloured, like that prepared in the common way, it is bitter, and extremely pale, when fresh made ; its taste much resembles that of the plant itself. With respect to the strength of extracts made *in vacuo*, I have not yet gained sufficient information to be able to present a view of the relative proportions which they bear to the common extracts ; but I have been informed by several medical friends who have given them a trial that they find them materially stronger. Perhaps some gentleman will consider the subject sufficiently deserving of investigation to collect such a statement of cases as will enable him to present to the Society the relative doses. I shall be glad to offer for the acceptance of such, specimens of any kind which they may be inclined to make use of."

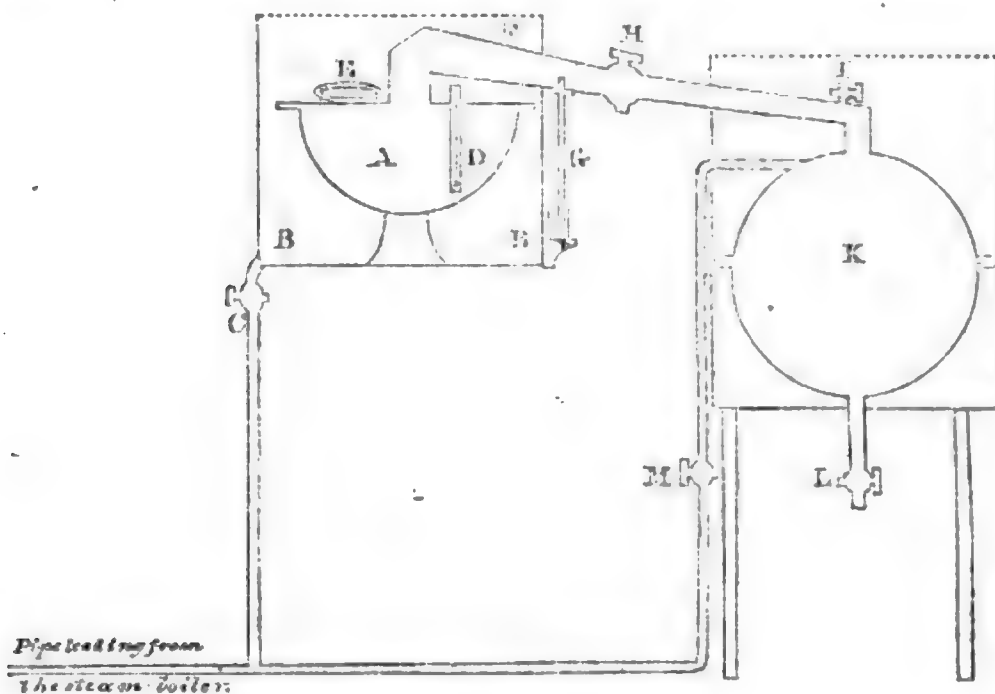
There is nothing new in the principle upon which Mr. Barry's apparatus acts. The patent is founded upon the method of forming a vacuum by means of steam, and of maintaining it by immersing the whole under water ; hence the apparatus, from its being thus rendered easy of management, and less expensive, appeared likely to be employed in other manufactures, such as sugar refining, and colour making. "Chemists, therefore," says Mr. B. "who are desirous of inspissating extracts *in vacuo* are at liberty to do it in any apparatus differing from that which has been made the subject of my patent, and thus these substances may continue the objects of fair competition as to quality and price."

The following observations and sketch will convey an idea of the form and mode of operation of Mr. Barry's apparatus.

"The mode of procuring the vacuum, and the manner of main-

taining it, have already been mentioned. The degree of exhaustion is of course judged of by the column which is supported in the attached mercurial gauge; and I think it will excite some surprise when it is stated, that although no pump is employed, yet that column is often at a height of 28 inches during rapid ebullition. In fact, it is common to operate with a column not two inches less than the barometer of the day, and at such times the temperature of the boiling fluid is below 100° Fahr.; often at 95°; and I am satisfied that by certain improvements, this temperature may be reduced to less than 90° in the ordinary process of manufacture. The vessels employed in the apparatus first put up were two: the one of cast-iron polished in its inner surface, serving as the evaporating pan, and situated in a water-bath, may be called a *still*. The head of it leads into a second vessel, which is a large copper sphere, about three times as large as the other, and surrounded at pleasure by cold water; it may be called the *receiver*. In the pipe which connects these two, is a large stop-cock, by which their communication with each other can be suspended. The manner of setting it to work is this: The juice, or infusion, is introduced through a large opening into the polished iron still, which is then closed, made air-tight, and covered with water. The stop-cock which leads to the receiver is also shut. In order to produce the vacuum, steam is allowed to rush through the copper sphere until it has expelled all the air, for which five minutes are commonly sufficient. This is known to be effected by the steam issuing uncondensed. At that instant the copper sphere is closed, and the steam shut off, and then cold water admitted upon its external surface. The vacuum thus produced in the copper sphere, which contains four-fifths of the air of the whole apparatus, is now partially transferred to the still by opening the intermediate stop-cock. Thus four-fifths of the air in the still rush into the sphere; and the stop-cock being shut again, a second exhaustion is effected by steam in the same manner as the first was; after which a momentary communication is again allowed between the iron still and the receiver: by this means four-fifths of the air remaining after the former exhaustion is expelled. These exhaustions repeated five or six times are usually found sufficient to raise the mercurial column to the height before mentioned. The water-bath in which the iron still is immersed is now to be heated until the fluid that is to be inspissated begins to boil, which is known by inspection through a window in the apparatus, made by fastening on air-tight a piece of very strong glass; and the temperature at which the boiling point is kept up is determined by a thermometer. Ebullition is continued until the fluid is inspissated to the proper degree of consistence, which also is tolerably judged of by its appearance through the glass window. I prefer making, for a single operation, as much juice or infusion as will keep the apparatus employed for nearly the

whole day. When inspissated sufficiently, the residuum, which we denominate extract, is taken out for use, which, if it has become too stiff, may be easily brought to a proper consistence by gently warming it, and kneading it with sufficient water."



- A. Iron still, or evaporating pan.
  - B. Water-bath.
  - C. Steam-pipe to heat this bath.
  - D. Thermometer indicating the internal temperature.
  - E. One of the covers in which is a glass aperture.
  - F. Pipe leading from the still to the receiver.
  - G. Mercurial column to measure the degree of exhaustion.
  - H. Stop-cock.
  - I. Cock for admission of air.
  - K. Receiver in the refrigerating vessel.
  - L. Cock for drawing off the condensed water.
  - M. Cock for admitting steam when the air is to be expelled.
- (Med. Chirurg. Trans. vol. x. p. 230.)

#### XVI. Cow-Pox in Persia.

Extract of a letter from William Bruce, Esq. resident at Bushire, to William Erskine, Esq. of Bombay, dated Bushire, March 26, 1813, communicating the discovery of a disease in Persia, contracted by such as milk the cattle and sheep, and which is a preventive of the small-pox.

MY DEAR SIR,

"When I was in Bombay I mentioned to you that the cow-pox was well known in Persia by the Eliaats, or wandering tribes: since my return here, I have made very particular inquiries on this subject amongst several different tribes who visit this place in the winter to sell the produce of their flocks, such as



carpets, rugs, butter, cheese, &c. Their flocks during this time are spread over the low country to graze; every Eliaat that I have spoken to on this head, of at least six or seven different tribes, has uniformly told me, that the people who are employed to milk the cattle caught a disease, which, after once having had, they were perfectly safe from the small-pox; that this disease was prevalent among the cows, and showed itself particularly on the teats: but that it was more prevalent, and more frequently caught from the sheep. Now this is a circumstance that has never, I believe, before been known; and of the truth of it I have not the smallest doubt, as the persons of whom I inquired could have no interest in telling me a falsehood, and it is not likely that every one whom I spoke to should agree in deceiving; for I have asked at least some 40 or 50 persons. To be more sure on the subject, I made most particular inquiries of a very respectable farmer, who lives about 14 miles from this, by name Mallilla (whom Mr. Babington knows very well), and who is under some obligations to me. This man confirmed every thing that the Eliaats had told me; and further said, that the disease was very common all over the country, and that his own sheep often had it. There may be one reason for the Eliaats saying that they caught the infection oftener from the sheep than the cow, which is, that most of the butter, ghee, cheese, &c. is made from sheep's milk, and that the black-cattle yield very little, being more used for draught than any thing else. If you think this information worthy of being communicated to the Society of which I have the honour of being a member, I beg you will do it in any way you think proper."—(Transactions of the Literary Society of Bombay.)

XVII. *Further Observations by S. in Answer to X.*

SIR,

London, Aug. 9, 1819.

In the number of the *Annals* for the present month, I observe a letter, signed X, in answer to some observations I formerly addressed to you.

As the writer, from his style, seems to consider his remarks as at once concise and masterly, or at least to have aimed at something which should be so, he may perhaps expect some kind of apology from me for venturing to differ from him in opinion. I must, however, assure him, that he has by no means succeeded in convincing me that I am wrong.

I have now no reason to expect that any thing useful will arise from prolonging the discussion. In what I have at present to state, I shall, therefore, be as brief as possible. I have already quoted, at full length, the passage from Dr. Clarke which I was desirous of having elucidated; but as your correspondent X. appears to have misunderstood the Doctor's meaning, I shall recal his attention to one sentence, and appeal to himself whether it will bear the interpretation he is pleased to put upon it.



The words of Dr. Clarke are, "*all the rolling CLOUDS disappeared, EXCEPTING a belt collected in FORM OF A RING highly luminous, around the moon;*" evidently implying, not (as X. would have it) that there was *the form, or appearance*, of a ring around the moon, but that there was a *cloud* (otherwise I know not the use of the word "*excepting*") encircling the moon *in the form or shape* of a ring. If X. means to assert that Dr. Clarke is not a perspicuous writer, the subject is immediately changed; but I am not of opinion, from the acquaintance that I have with Dr. Clarke's works, that there is room for supposing that he wrote otherwise than he meant.

All that I attempted to show in my former letter was, that this appearance could not arise from a cloud situated as above described. I will now further observe that it must have been by a most extraordinary coincidence—a coincidence indeed hardly to be believed, that a *cloud* lying wholly between the moon and the spectator presented such an appearance as that depicted by Dr. Clarke in p. 488 of his volume. It was on this account that I requested Dr. Clarke, or some other of your intelligent correspondents, to suggest some more satisfactory principle on which the phenomenon might be accounted for. I have failed to obtain such information as I desired; for the Doctor himself seems to have viewed this appearance of the moon in no other light than as a beautiful spectacle, or, *perhaps*, as it might make a figure in his quarto; and as to any other quarter from which information may be derived, it is hardly to be expected that any person should set about explaining what he never saw. It appears then upon the whole that I must be content to believe that such a phenomenon was *once* seen, but from what cause remains, and is likely to remain, one of the unfathomable secrets of nature.

I cannot conclude these the last remarks I shall make on this question, without observing, that although I am now no more enlightened on the subject than at the date of my last letter, still the *desire* of obtaining information, by whomsoever expressed, on subjects which excite the doubts of the inquirer, can be considered by X. alone as "*too ludicrous to merit further notice.*"

I am, Sir, your obedient servant,

S.

#### XVIII. *Mathematical Problem.* By Mr. Adams.

(To Dr. Thomson.)

SIR,

Stonchouse, Sept. 4. 1819.

On reconsidering problem 4, Art. IV, of the *Annals of Philosophy* for March, 1819, I find that a small alteration in the formula there given will reduce the solution of the problem to one general rule, the insertion of which, in some future number of your journal, will much oblige, Sir,

Your most obedient servant,

JAMES ADAMS.

The formula above alluded to is

$$\cos. S M = \cos. C - \frac{2 \sin. A . \sin. B . \cos. S H . \cos. M R}{\cos. m R . \cos. s H} = \cos. C - \frac{2 M}{N}.$$

$$\text{Since } \cos. \frac{1}{2} S M = \sqrt{\frac{1 + \cos. S M}{2}} \text{ and } \cos. \frac{1}{2} C = \sqrt{\frac{1 + \cos. C}{2}},$$

we have

$$\cos. S M = 2 \cos.^2 \frac{1}{2} S M - 1, \text{ and } \cos. C = 2 \cos.^2 \frac{1}{2} C - 1,$$

then by substitution,

$$2 \cos.^2 \frac{1}{2} S M - 1 = 2 \cos.^2 \frac{1}{2} C - 1 - \frac{2 M}{N}, \text{ from whence } \cos.^2$$

$$\frac{1}{2} S M =$$

$$\cos.^2 \frac{1}{2} C - \frac{M}{N} = \left(1 - \frac{M}{N \cos.^2 \frac{1}{2} C}\right) \cos.^2 \frac{1}{2} C. \text{ Put } \sin.^2 D =$$

$$\frac{M}{N \cos.^2 \frac{1}{2} C},$$

$$\text{then will } \cos.^2 \frac{1}{2} S M = (1 - \sin.^2 D) \cos.^2 \frac{1}{2} C = \cos.^2 D .$$

$$\cos.^2 \frac{1}{2} C, \text{ therefore,}$$

$$\cos. \frac{1}{2} S M = \cos. D . \cos. \frac{1}{2} C. \text{ But } 2 \log. \sin. D = \log.$$

$$\frac{M}{N \cos.^2 \frac{1}{2} C}, \text{ therefore,}$$

$$\log. \sin. D = \frac{1}{2} \log. \frac{M}{N \cos.^2 \frac{1}{2} C}. \text{ Hence the following rule :}$$

#### Rule.

Log. sin. A.....	=
sin. B.....	=
cos. *'s true alt.....	=
cos. D's true alt.....	=
cos. *'s app. alt. A . C.....	=
cos. D's app. alt. A . C.....	=
2 log. cos. $\frac{1}{2}$ C    A . C.....	=
Sum of logarithms.....	=
$\frac{1}{2}$ sum corresponding to sin. D.....	=
cos. D.....	=
cos. $\frac{1}{2}$ C.....	=
cos. $\frac{1}{2}$ true distance.....	=

Borda's formula, as given in Dr. Gregory's Trigonometry, gives the sin.  $\frac{1}{2}$  true distance, and has the same number of operations as in the above rule, but the demonstrations are different.



XIX. *Urinary Concretion on a Leaden Pipe.*

In the Glasgow Infirmary, there is a lead pipe about three inches in diameter, through which all the urine collected in the hospital has been discharged for upwards of 20 years. The inside of the pipe was encrusted with a matter, somewhat similar to calc tuff, and above an inch thick. I had the curiosity to examine this substance in order to ascertain its composition.

Twenty-five grains of it were reduced to a coarse powder, and boiled for two hours in distilled water. The liquid was then passed through a filter, and the undissolved matter dried at the temperature of  $212^{\circ}$ . It weighed 19.7 gr. and had exactly the same appearance as at first.

The water being evaporated left one gr. of a brown coloured matter, which, when exposed to a heat of  $212^{\circ}$ , gave out traces of ammonia, and became insoluble in water. Nitric acid readily dissolved it, and converted it into bitter; but I could obtain no traces of oxalic acid. The soluble portion of course contained urea. The greater part of this matter had been decomposed by the boiling. Hence the loss of weight sustained.

The 19.7 gr. were dissolved in muriatic acid. There remained undissolved 1.1 gr. of matter, chiefly siliceous sand, but containing also a brown substance, which dissolved with effervescence in nitric acid, becoming yellow and gelatinous. Probably, therefore, it was albumen.

The muriatic acid solution, being precipitated by ammonia, let fall 5.8 gr. of phosphate of lime. Carbonate of ammonia afterwards threw down 2.8 gr. of carbonate of lime.

The matter deposited in the lead pipe dissolved with effervescence in muriatic acid, and, therefore, contained carbonate of lime.

Thus the 25 gr. of the incrustation yielded the following substances :

Urea, &c.....	1.0
Albumen and sand .....	1.1
Phosphate of lime. ....	5.8
Carbonate of lime. ....	2.8
	<hr/>
	10.7

I believe 4.3 gr. of urea to have been decomposed during the analysis; so that the deficiency amounts to 10 gr. Probably the greatest part of this is water. It is very remarkable that I could detect no soluble saline matter in this incrustation.

This analysis seems to prove that carbonate of lime is one of the constituents of human urine.

XX. *Aurora Borealis*, observed by Dr. W. Burney.

Gosport Observatory, Oct. 20, 1819.

On the 17th instant, at seven, p.m. a light about  $30^{\circ}$  on either side of the magnetic north point appeared in the shape of a luminous arch, whose apex was  $18^{\circ}$  above the horizon; and in about 20 minutes after, *three* beautiful perpendicular columns of flame-coloured light sprang up in the N. by W. point. They were at a short distance from each other, and about  $16^{\circ}$  high; their tops spread out considerably in very fine curved streams, and the bottom parts exhibited a dark-blue colour. While passing off steadily in a southern course, several shorter columns appeared, and followed exactly in the same direction, the wind being at that time fresh from the northward, and having some influence upon their motion.

At a quarter before eight, several other columns appeared; they also passed off slowly, and were immediately followed by a small brilliant meteor. At half-past eight, *two* more perpendicular columns sprang up about N.W. by N. one  $4^{\circ}$ , and the other  $9^{\circ}$  long, and  $2^{\circ}$  broad: these, as well as the others, passed under the northern crown, and finally disappeared at due W. and were also followed by three brilliant meteors, which fell almost perpendicularly. Behind the last two columns there were six small tapering lights nearly equidistant, and about  $4^{\circ}$  or  $5^{\circ}$  above the horizon; but which, in a higher northern latitude, would have had a beautiful appearance. At a quarter before nine o'clock, the last column of light appeared at W.N.W. and moved off slowly in the same direction as the others; and at nine, another meteor, with a long sparkling train, fell through a great space in a westerly direction. Soon after this, the luminous arch in the northern hemisphere entirely disappeared, and some haze collected near the horizon. At nine o'clock, the thermometer stood at  $40^{\circ}$ , and sunk only  $1^{\circ}$  during the night; but according to the nocturnal diminution of temperature in this neighbourhood with a northerly wind, it should have sunk  $6^{\circ}$  lower. The barometric column too, which had been rising, now began to sink slowly: these circumstances, together with the meteoric *phenomena* and dew in the night, which seldom falls during a dry northerly wind, certainly indicate that the lower stratum of air had received a considerable addition of electric matter from the appearance of the northern lights. In 60 hours after these lights, the barometric column had sunk three-fifths of an inch, when a gale of wind came on from the S. W. with heavy rain; a circumstance that frequently occurs after their appearance.

ARTICLE XII.

*Astronomical, Magnetical, and Meteorological Observations.*

By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*

Latitude 51° 37' 42" North. Longitude West in time 1' 20.7".

*Astronomical Observations.*

Sept. 6.	Immersion of Jupiter's fourth satellite .....	{ 9 <sup>h</sup> 16' 01"	Mean Time at Bushey.
		{ 9 17 22	Mean Time at Greenwich.
12.	Emersion of Jupiter's first satellite .....	{ 9 22 17	Mean Time at Bushey.
		{ 9 23 38	Mean Time at Greenwich.
18.	Emersion Jupiter's third satellite .....	{ 9 37 44	Mean Time at Bushey.
		{ 9 39 05	Mean Time at Greenwich.
19.	Emersion of Jupiter's first satellite .....	{ 11 18 41	Mean Time at Bushey.
		{ 11 20 02	Mean Time at Greenwich.
28.	Emersion of Jupiter's first satellite .....	{ 7 44 48	Mean Time at Bushey.
		{ 7 46 09	Mean Time at Greenwich.

*Magnetical Observations, 1819. — Variation West.*

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
Sept. 1	8 <sup>h</sup> 30'	24°	27' 40"	1 <sup>h</sup> 25'	24°	40' 54"	6 <sup>h</sup> 55'	24°	33' 02"
2	8 30	24	31 46	1 25	24	42 12	6 55	24	34 00
3	8 45	24	32 12	1 15	24	44 02	—	—	—
4	8 35	24	30 30	1 25	24	41 54	6 50	24	29 45
5	—	—	—	1 25	24	44 38	6 45	24	35 23
6	8 45	24	30 14	1 25	24	43 24	6 45	24	33 27
7	8 35	24	34 51	1 15	24	44 14	6 45	24	34 54
8	8 35	24	31 54	1 25	24	41 42	6 45	24	34 56
9	8 40	24	30 47	1 25	24	40 50	—	—	—
10	8 35	24	32 06	1 30	24	43 06	6 45	24	32 54
11	8 35	24	34 07	1 25	24	40 51	6 35	24	33 51
12	8 35	24	33 11	1 45	24	39 54	6 30	24	33 43
13	8 30	24	30 03	1 30	24	40 52	6 35	24	34 09
14	8 35	24	32 01	1 20	24	39 32	6 30	24	34 03
15	8 35	24	31 53	1 10	24	40 23	6 15	24	36 54
16	8 35	24	30 50	1 25	24	37 58	6 30	24	33 08
17	8 40	24	34 04	1 45	24	38 45	6 20	24	35 17
18	8 35	24	32 35	1 25	24	44 25	—	—	—
19	8 35	24	36 13	1 25	24	40 52	6 20	24	27 05
20	8 35	24	33 06	1 30	24	42 37	6 15	24	34 22
21	8 35	24	33 56	1 20	24	41 29	6 15	24	32 43
22	8 35	24	35 48	1 20	24	39 39	—	—	—
23	8 40	24	32 54	1 35	24	40 29	—	—	—
24	8 35	24	33 32	1 35	24	41 32	6 10	24	32 21
25	8 25	24	34 15	1 45	24	44 23	6 10	24	30 08
26	8 40	24	31 25	1 10	24	40 21	6 05	24	36 33
27	8 40	24	33 50	1 30	24	44 59	6 05	24	34 33
28	8 35	24	32 26	1 30	24	41 52	6 05	24	32 34
29	—	—	—	1 25	24	40 18	6 05	24	33 18
30	8 35	24	31 12	1 30	24	39 40	6 00	24	34 09
Mean for the Month.	} 8 36	24	32 29	1 26	24	41 35	6 27	24	33 27



## Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six'th.
Sept.		Inches.				Feet.		
1	Morn....	29.058	51°	62°	WNW		Very fine	48
	Noon....	29.092	58	50	WNW		Very fine	61
	Even....	29.165	53	54	W		Very fine	45
2	Morn....	29.230	52	64	W		Fine	
	Noon....	29.253	61	47	W		Fine	64½
	Even....	29.260	60	57	W		Showery	59
3	Morn....	29.180	63	94	W by S		Showery	
	Noon....	29.276	70	64	W		Cloudy	71
	Even....	—	—	—	—		—	53
4	Morn....	29.455	58	84	WSW		Very fine	
	Noon....	29.496	69	57	SSW		Fine	69½
	Even....	29.446	64	68	SSW		Rain	59
5	Morn....	29.329	60	89	WNW		Rain	
	Noon....	29.370	64	75	W by S		Fine	67
	Even....	29.393	60	63	W by S		Fine	48
6	Morn....	29.484	55	67	WNW		Very fine	
	Noon....	29.538	62	52	WNW		Very fine	65½
	Even....	29.590	53	55	W		Very fine	50½
7	Morn....	29.585	61	90	SW		Mizzle	
	Noon....	29.590	67	78	SW		Cloudy	68
	Even....	29.600	65	77	W by S		Fine	61
8	Morn....	29.670	63	75	W		Fine	
	Noon....	29.670	71	62	W		Fine	71½
	Even....	29.695	65	67	W		Cloudy	61
9	Morn....	29.695	66	70	S by E		Cloudy	
	Noon....	29.693	71	56	SSW		Fine	73½
	Even....	—	—	—	—		—	58
10	Morn....	29.660	63	85	NE		Fine	
	Noon....	29.648	72	51	SE		Fine	72½
	Even....	29.630	64	63	Calm		Very fine	52½
11	Morn....	29.617	58	92	NNE		Foggy	
	Noon....	29.681	61	79	NNE		Cloudy	62½
	Even....	29.700	60	74	NNE		Cloudy	53
12	Morn....	29.800	57	63	NNE		Fine	
	Noon....	29.800	65	52	ENE		Very fine	66½
	Even....	29.840	59	56	NE		Very fine	48½
13	Morn....	29.845	56	68	E by N		Clear	
	Noon....	29.843	66	51	ENE		Very fine	67
	Even....	29.842	60	57	ESE		Very fine	51
14	Morn....	29.802	61	50	E		Foggy	
	Noon....	29.779	70	51	ESE		Clear	71½
	Even....	29.734	65	58	E by S		Clear	57
15	Morn....	29.546	64	70	E		Very fine	
	Noon....	29.453	73	56	SE		Showery	74½
	Even....	29.400	65	81	SW		Cloudy	52
16	Morn....	29.255	53	89	NNW		Rain	
	Noon....	29.305	53	78	NNW		Fine	56
	Even....	29.338	51	67	N by W		Cloudy	43½
17	Morn....	29.559	52	76	NNE		Very fine	
	Noon....	29.620	60	56	NNE		Fine	60½
	Even....	29.672	54	65	Calm		Cloudy	49½
18	Morn....	29.750	55	77	WSW		Cloudy	
	Noon....	29.738	64	63	W		Fine	65
	Even....	—	—	—	—		—	

*Meteorological Observations continued.*

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Sept.		Inches.				Feet.		
19	Morn....	29.738	55°	68°	NNW		Very fine	55°
	Noon....	29.745	57	59	NNW		Cloudy	59½
	Even....	29.745	50	58	NE		Very fine	41½
20	Morn....	29.861	48	71	N by E		Clear	41½
	Noon....	29.880	58	55	NE		Fine	59½
	Even....	29.900	52	59	NE		Very fine	42
21	Morn....	30.010	50	74	NE by N		Very fine	61
	Noon....	30.015	57	55	NE		Fine	42½
	Even....	30.005	54	64	NE		Fine	58½
22	Morn....	29.985	51	72	NE		Very fine	50
	Noon....	29.950	57	61	E		Cloudy	60½
	Even....	—	—	—	—		—	50½
23	Morn....	29.779	54	69	E		Cloudy	63
	Noon....	29.685	60	62	E		Cloudy	51½
	Even....	—	—	—	—		—	60½
24	Morn....	29.433	54	79	ENE		Cloudy	51½
	Noon....	29.368	62	61	SE		Fine	55½
	Even....	29.287	55	64	ESE		Very fine	58
25	Morn....	29.154	56	88	ESE		Rain	49
	Noon....	29.088	60	91	S by E		Rain	62
	Even....	—	55	82	SSW		Cloudy	55½
26	Morn....	29.050	54	87	WSW		Cloudy	61
	Noon....	29.110	55	80	WSW		Rain, thund.	54½
	Even....	29.190	52	71	SW		Very fine	60
27	Morn....	29.139	55	90	W by S		Fine	54½
	Noon....	29.164	60	89	SW		Showery	65½
	Even....	29.192	59	83	SW		Fine	61
28	Morn....	29.138	60	88	SW by S		Rain	54½
	Noon....	29.182	60	84	SW by S		Showery	60
	Even....	29.200	57	85	SSW		Cloudy	54½
29	Morn....	29.121	—	94	SSW		Hard rain	60
	Noon....	29.138	59	93	WSW		Cloudy	54½
	Even....	29.252	55	78	S by W		Fine	65½
30	Morn....	29.334	60	88	SW		Rain	65½
	Noon....	29.362	65	73	SW		Cloudy	65½
	Even....	29.375	61	82	SSW		Fine	

Rain, by the pluviometer, between noon the 1st of September, and noon the 1st of October, 3.213 inches. Evaporation, during the same period, 3.55 inches.

ARTICLE XIII.

METEOROLOGICAL TABLE.

1819.	Wind.		BAROMETER		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
			Max.	Min.	Max.	Min.			
9th Mo.									
Sept.	1	N W	29.74	29.49	66	45	—		55
	2	N W	29.74	29.69	70	58	—	11	57
	3	W	29.94	29.70	76	51	—	13	91
	4	S W	29.94	29.86	74	59	54	08	71
	5	N W	29.96	29.86	66	47	—	05	92
	6	N W	30.07	29.96	70	48	—		63
	7	W	30.14	30.07	72	66	—		76
	8	S W	30.15	30.14	78	52	39		69
	9	S W	30.11	30.10	79	59	—		69
	10	E	30.12	30.08	80	45	—		72
	11	N E	30.24	30.12	66	55	—		66
	12	N E	30.29	30.24	70	39	—		61
	13	S E	30.30	30.27	73	42	50		61
	14	S E	30.29	30.03	77	45	—		79
	15	E	30.03	29.78	82	52	—	36	67
	16	N	30.03	30.00	56	42	—	09	72
	17	N	30.23	30.03	65	46	—		65
	18	W	30.24	30.23	70	45	46		67
	19	N	30.35	30.24	60	33	—		58
	20	N	30.50	30.35	63	38	—		65
	21	N	30.50	30.50	66	36	—		64
	22	N E	30.50	30.30	68	46	40		65
	23	N E	30.30	29.95	65	50	—		63
	24	E	29.95	29.67	68	40	—	03	68
	25	Var.	29.67	29.60	66	50	—	33	96
	26	S W	29.67	29.61	64	48	—	11	74
	27	W	29.70	29.66	65	57	—	40	74
	28	S W	29.70	29.65	67	54	—	66	77
	29	S W	29.84	29.65	66	55	—	23	90
	30	S W	29.89	29.85	69	60	53		80
			30.50	29.49	82	33	2.82	2.58	55—91

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Ninth Month.*—1. Fine. 2. Clouds: rain, evening. 3. Fine: *Cirrus*: rainy evening. 4. Cloudy evening. 5. Rain, morning: fine, p. m. 6, 7. Cloudy. 8. *Cirrocumulus*. 9. *Stratus* in the morning: day, cloudy: fair. 10—13. Fine. 14. *Stratus*, followed by *Cirrus*. 15. Much dew: *Cirrocumulus*. 16. Rainy morning: fine, p. m.: *Cirrocumulus*. 17—20. Fine. 21. *Cirrocumulus*, with *Cirrostratus*. 22. *Cirrus*, *Cirrocumuli*. 23. Overcast sky. 24. *Cirrocumulus*. 25. Rainy. 26—29. Showery. 30. Cloudy. Considerable wind (by night especially), has accompanied the late depression of the barometer.

## RESULTS.

Winds: N, 5; NE, 5; E, 2; SE, 2; SW, 7; W, 4; NW, 4; Var. 1.

## Barometer: Mean height

For the month. ....	30·013 inches.
For the lunar period ..	29·999
For 13 days, ending the 5th (moon south) .....	30·183
For 14 days, ending the 19th (moon north) .....	30·138

## Thermometer: Mean height

For the month.....	59·00°
For the lunar period .....	59·155
For 30 days, the sun in Virgo.....	60·30

Hygrometer: Mean for the month . . . . . 70·9

Evaporation. .... 2·82 inches.

Rain ..... 2·65

Rain at Tottenham..... 2·63

On the 25th, at seven, a. m. I observed at Tottenham two *parhelia*, formed in portions of a large halo, which were seated in a body of haze suspended over a *Nimbus*: they lasted but a few minutes, and were followed by wind and rain.

Stratford, Tenth Month, 16, 1819.

L. HOWARD.

# ANNALS OF PHILOSOPHY.

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DECEMBER, 1819.

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## ARTICLE I.

*On the Figure of the Earth.\** By M. de Laplace. (Read at the Meeting of the Board of Longitude on May 26, 1819.)

THE numerous experiments on the pendulum have shown that the increase of gravitation follows a very regular law, and that it is very nearly proportional to the square of the sine of the latitude. This force being the result of the attractions of all the particles of the earth, these observations compared with the theory of the attractions of spheroids, furnish the only means which we have to penetrate into the interior constitution of the earth. It follows from them that this planet is formed of beds whose density increases from the surface to the centre, and which are disposed regularly round this point. I published at the end of the *Connaissance des Temps* for 1821 the following theory, which I have demonstrated in the second volume of the *New Memoirs of the Academy of Sciences*.

“ If we take for unity the length of the seconds pendulum at the equator, and if to the length of this pendulum observed at any point of the surface of the terrestrial spheroid, we add half the height of this point above the level of the ocean, divided by the demiaxis of the pole—a height which the observation of the barometer furnishes, the increase of this length thus corrected will be, on the hypothesis of a constant density below a moderate depth, equal to the product of the square of the sine of the latitude by five-fourths of the ratio of the centrifugal force to that of gravitation at the equator, or by 43 ten millionths.”

\* Translated from the *Ann. de Chim. et de Phys.* xi. 31.



This theorem is generally true, whatever be the density of the sea and the manner in which it covers a part of the earth.

The experiments with the pendulum made in the two hemispheres agree in giving to the square of the sine of the latitude a greater coefficient, very nearly equal to 54 ten millionths. It is then proved by these experiments that the earth is not homogeneous in its interior, and that the density of its strata increases from the surface to the centre.

But the earth, though heterogeneous in a mathematical sense, would be homogeneous in a chemical sense, if the increase of the density of these beds were owing merely to the increase of pressure which they experience in proportion as they approach the centre. We may conceive in fact that the immense weight of the superior beds may increase their density considerably, even supposing them not to be fluid; for we know that solid bodies are compressed by their own weight. The law of the densities resulting from this compression being unknown, we cannot determine how much the density of the strata of the earth may be thus increased. The pressure and the heat which we can produce are always very small when compared with those which exist at the surface and in the interior of the sun and stars. It is even impossible for us to have an approximate idea of the effects of these forces united in these great bodies. Every thing leads to the notion that they existed at first in a great degree in the earth, and that the phenomena which they have produced, modified by their successive diminution, form the actual state of the surface of our globe—a state which is only an element of the curve, of which time will constitute the abscissa, and whose ordinates will represent the changes which that surface undergoes without ceasing. We are far from knowing the nature of this curve. We cannot, therefore, deduce with certainty the origin of what we see on the earth; and if, to satisfy the imagination, always uneasy at its ignorance of the cause of the phenomena which interests us, we hazard some conjectures; it is the part of a wise man to state them with the greatest caution.

The density of any gas is proportional to its compression, when the temperature remains the same. This law found accurate within the limits of the density of gases which we are able to examine, cannot, as is obvious, apply to liquids and solids, whose density is very great when compared with that of gases, when the pressure is small or nil. It is natural to think that these bodies make the greater resistance to compression the more they are compressed; so that the ratio of the differential of the pressure to that of the density, instead of being constant as in the gases, increases with the density. The most simple function which can represent this ratio is the first power of the density multiplied by a constant quantity. I have adopted it because it has the advantage of representing in the simplest

manner what we know respecting the compression of liquids and solids, and applies easily to the calculus in researches respecting the figure of the earth. Hitherto mathematicians have not taken into consideration the effect resulting from the compression of the strata. Dr. Young has just drawn their attention to this subject by the ingenious remark, that we may explain in this manner the increase of the density of the strata of the terrestrial spheroid. I expect that the following analysis\* will be seen with some interest, from which it results that it is possible in this way to explain all the known phenomena depending on the law of the density of these strata. These phenomena are: the variations of the degrees of the meridian and of gravitation; the precession of the equinoxes; the nutation of the terrestrial axis; the inequalities which the flattening (*aplatissement*) of the earth produces in the motion of the moon; finally, the ratio of the mean density of the earth to that of water—a ratio which Cavendish fixed by a very beautiful set of experiments at  $5\frac{1}{4}$ . Setting out from the preceding law of the compression of liquids and solids, I find that if we suppose the earth formed of a homogeneous substance, in the chemical sense of the word, whose density is  $2\frac{1}{4}$  to that of common water, and compressed by a vertical column of its own substance equal to the millionth part of the polar axis, if its density increases  $5\cdot5345$  millionths of its primitive density, we account for all these phenomena. The existence of such a substance is very admissible, and there are probably such substances at the surface of the earth.

If the globe were entirely formed of water, and if we suppose, according to the experiments of Canton, that the density of water at the temperature of  $10^{\circ}$  ( $50^{\circ}$  Fahr.) and compressed by a column of water 10 metres in height, increases 44 millionths, the flattening of the earth will be  $\frac{1}{3860}$ , the coefficient of the square of the sine of the latitude in the expression for the length of the seconds pendulum will be 59 ten thousandths, and the mean density of the earth will be nine times that of water. All these results deviate from observations beyond the limits of the errors of which they are susceptible.

I suppose the temperature uniform in the whole extent of the terrestrial spheroid; but it is possible that the heat may be greater towards the centre; and this will be the case on the supposition that the earth, possessing originally a great degree of heat, cooled gradually. Our ignorance of the interior constitution of this planet does not allow us to calculate the law of that cooling, and the diminution which results from it in the mean temperature of climates; but we may establish with certainty that this diminution has been insensible for 2000 years.

Let us suppose in a space whose temperature is constant, a sphere endowed with a rotatory motion; let us suppose then

\* This analysis will appear in the volume of the *Connaissance des Temps* for 1822, at present in the press.

that after a long time the temperature of the space diminishes one degree ; the sphere will ultimately assume this new degree of temperature ; its mass will not be altered ; but its dimensions will diminish by a quantity which I suppose a hundred thousandth part, as is nearly the case with glass. In virtue of the principle of areas, the sum of the areas, which each molecule of the sphere describes round its axis of rotation, will be in a given time the same as before. It is easy to conclude from this, that the angular velocity of rotation will be increased a 50 thousandth part. Therefore supposing the duration of rotation to be one day, or 100,000 decimal seconds, it will be diminished two seconds by the diminution of one degree in the temperature of the space. If we extend this consequence to the earth, and if we consider that the length of the day has not varied since Hipparchus so much as the hundredth of a second, as I have shown by a comparison of observations with the theory of the secular equation of the moon, we shall conclude that since that time the variation of the internal heat of the earth is insensible. It is true indeed that dilatation, specific heat, the greater or smaller permeability to heat, and the density of the different strata of the terrestrial spheroid, all of them unknown, may occasion a sensible difference between the results relative to the earth and those of the sphere which we have just considered, according to which a diminution of the hundredth of a second in the duration of the day answers to a diminution of a two hundredth of a degree in the temperature. But this difference, corresponding to the diminution of an hundredth of a second in the length of the day, can never raise the loss of terrestrial heat from the two hundredth of a degree to a tenth. We see even that the diminution of a hundredth of a degree near the surface supposes a greater diminution in the temperature of the inferior strata ; for we know that at length the temperature of all the strata diminishes in the same geometric progression ; so that the diminution of a degree near the surface corresponds to greater diminutions in the beds nearer the centre. The dimensions of the earth and its vis inertiae diminish, therefore, more than in the case of the sphere which we have imagined. It follows from this, that if in the course of time we observe any change in the mean height of the thermometer placed at the bottom of the caverns below the observatory, we must ascribe it not to any variation in the mean temperature of the earth, but to a change in the climate of Paris, the temperature of which may vary from many accidental causes. It is remarkable that the discovery of the true cause of the secular equation of the moon makes us acquainted at the same time with the invariability of the length of the day, and that of the mean temperature of the earth, from the epoch of the most ancient observations.

This last phenomenon leads us to think that the earth has now come to the permanent state of temperature which corre-

sponds with its position in space, and relatively to the sun. We find by analysis that whatever be the specific heat, the permeability to heat, and the density of the strata of the terrestrial spheroid, the increase of heat at a very small depth when compared with the radius of that spheroid, is equal to the product of this depth by the elevation of the temperature of the surface of the earth above the state of which I have just spoken, and by a factor independent of the dimensions of the earth, which depends only on the qualities of its first stratum relative to heat. From what we know of these qualities, we see that if this elevation amounted to several degrees, the increase of heat would be very sensible at the depths to which we have penetrated, and where, however, observations have not enabled us to discover it.

*Note by M. Arago.*

We conceive that our readers will not be displeased to find here some details respecting the method by means of which M. Laplace has established the constancy of the duration of the day.

A mean solar day is equal to the time which the earth employs to make a complete revolution on itself, increased by the mean apparent motion of the sun in the same interval. Theory has proved that the mean apparent motion of the sun, as that of all the planets, is constant. The length of the solar day then can vary only by a change in the velocity of the rotation of the earth.

We call *lunar month* the interval of time which the moon employs to return to the same position relatively to the sun ; to its conjunction, for example. This interval is evidently independent of the velocity of the rotation of the earth. Even though our globe were to cease altogether to turn on its axis, the movement of translation of the moon would experience no alteration. From this flows a very simple method of discovering if the duration of the solar day has changed.

Suppose that we determine at present by direct observations the duration of a lunar month ; that is to say, how many days and fractions of a day the moon employs to return to its conjunction with the sun. It is clear that by repeating this observation at another epoch, we should find a different result, if the length of the day has not been constant, even though the velocity of the moon has not changed during the interval. The month, for example, would appear longer if the duration of the day had diminished ; and shorter, on the contrary, if the day had become longer. The constancy of the lunar month will be a proof of the invariability of the duration of the day.

But all the observations concur to prove that from the Chaldeans to our time the duration of the lunar month has been gradually diminishing. Hence it follows from what has been said, either that the velocity of the moon has accelerated, or that



the solar day has become longer. But M. Laplace has discovered by theory that in the motion of the moon there is an inequality known by the name of *secular equation*, which depends on the variation of the eccentricity of the terrestrial orbit, and the value of which in each century may be deduced from the change of the eccentricity. By means of this equation we give a complete account of the increase of velocity in question. There is, therefore, no reason for supposing that the length of the day has not remained constant.

Let us admit for a moment, as Laplace does, that this duration surpasses at present the hundredth of a decimal second, that of the time of Hipparchus. The length of the century at present, or of 36525 solar days, will then be longer than it was 2000 years ago (it is known that Hipparchus lived about 120 years before our era) by 365.25'' decimal. In this interval of time, the moon describes an arc of 534.6'' decimal. This quantity then will express the difference between the arcs passed over by the moon in a century at the present time and at the time of Hipparchus; but as these arcs, determined by observations and corrected for the secular equation, do not differ by so great a quantity, we must conclude from it, that in this long interval, the duration of the day has not varied by the hundredth part of a decimal second.

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## ARTICLE II.

*Essay on the Turquoise and the Calaité.* By Dr. Gotthelf Fischer, Professor of Natural History in the University of Moscow.

THE term *turquoise* has been applied to two very different substances. The one, distinguished by the name of *oriental turquoise*, is a true stone, a clay coloured by oxide of copper, or even by arseniate of iron; and belongs as much to the argillaceous order of the oryctognostic system as chrysoprase belongs to the siliceous order. I have placed it in the system under the name of *calaité*, by which it had been already distinguished by Pliny. The other substance, called simply *turquoise*, or *occidental turquoise*, or *turquoise odontolite*, is a fossil, a petrefaction, a tooth or a bone coloured by a metallic phosphate, which does not belong to the mineral kingdom at all. Every part of the skeleton may be in this way converted into turquoise, when it happens to be placed in contact with coppery bodies, and particularly with phosphate of copper; but the fossil turquoise capable of being employed in the arts is almost always a tooth, which is harder than the other bones of the skeleton, and takes



an excellent polish. I shall distinguish it by the name of *turquoise odontolite*.

It is not surprising that the *mineral turquoise* or *calaité* has not been hitherto placed among stony bodies. The reason is, that most of them come to Europe already polished, and in very small pieces, and that most naturalists have considered it, with Reaumur, as merely a tooth coloured by copper.

That substance was, however, known to the ancients; and Pliny has described it pretty well under the name of *calaité*, or *borea*, in his chapter on opaque blue gems (lib. 37, c. 8). The following are the passages of that naturalist which relate to it. “*Calais e viridi pallens. Nascitur post aversa Indiæ, apud incolas Caucasi Montis, Phicaros et Asdatas, amplitudine conspicua, sed fistulosa ac sordium plena, sincerior multo præstantiorque in Caramaria. Utrobique in rupibus inviis, et gelidis, oculi figura extuberans, leviterque adhærens, nec ut agnata petris, sed ut apposita.*” Pliny speaks pretty correctly with respect to the position of this mineral. We should say at present, *calaité* is found in round pieces of the size and shape of the eye in alluvial lands between beds of clay; *non agnata petris*, not disseminated in a rocky matrix. Further on he compares it to the emerald, which certainly was not the gem known by that name at present.† “*Optimus color smaragdi; ut tamen apparet ex alieno est, quod placeant. Inclusæ decorantur auro, aurumque nullæ magis decent;*” or with his saphyr, as in cap. x. “*Calais sapphirum imitatur, candidior, et litteroso mari similis.*”

There can be no doubt that these passages refer to the mineral turquoise; especially when we consider that the comparisons of Pliny do not always refer to the colour, but to the general value, as was the manner of the Greeks.‡ Thus Pliny places a species of *calaité* in the fourth rank. “*Quarta apud eos (Græcos) vocatur borea, cælo autumnali matutino similis, et hæc erit illa (varietas calaidis) quæ vocatur ærizusa.*”

Tavernier had an exact idea of the mines of *calaité*, without however characterising the substance itself. He assures us that “in the east there are only two mines of turquoise known; the one the old rock, three days’ journey from Mahed, towards the north-west; the other the new rock, at the distance of five days’

\* In Greek we find *καλαίς* and *καλλαις*; hence the reason why some editors of Pliny write *callais*.

† I do not mean to say that Pliny was unacquainted with the emerald. Its colour and beauty are well expressed by these words (l. c. cap. v.): “*Tertia auctoritas smaragdis perhibetur pluribus de causis. Nullius coloris aspectus jucundior est. Nam herbas quoque virentes frondesque avide spectamus. Smaragdus vero tanto libentius quoniam nihil omnino viridius comparatum illis viret.*” Pliny assures us himself that we must not take this comparison in a strict sense by adding, “*ut tamen apparet ex alieno est.*”

‡ “*Sed minus refert nationes (istas gemmas gerentes) quam bonitates distinguere. Optima ergo, quæ purpuræ quicquam habet, secunda quæ rosæ, tertia quæ smaragdi. Singulis autem Græci nomina ex argumento dedere.*” Plin. *ibid.* versus finem.

journey. Those of the new rock are of a bad blue, and but little valued; as many of them as we choose may be obtained for little money; while for some years the king of Persia has forbid the old rock to be dug, except for his own use."

It appears to me astonishing that Reaumur did not subject these oriental turquoises to an analysis, or at least to a comparison with those of *Simore*, knowing that the ambassadors sent by the king of Persia to Louis XIV. brought among their presents a great many turquoises, which appear to have been all from the new rock, as their colour inclines to white. Reaumur wished to explain every thing by the objects which the mines of Languedoc furnished him with.

If Haüy, in his valuable work, seems fully to confirm the ideas of Reaumur by saying: "On trouve des dents molaires ou autres parties osseuses d'animaux, pénétrées de molécules cuivreuses, qui leur donnent une couleur bleue et quelquefois d'un bleu-verdatre. Les premiers ont été apportées de Turquie, ce qui a fait donner à cette substance le nom de turquoise," it is not surprising that the *calaite*, the true stone which comes from Persia, has not yet obtained a place in the systems of mineralogy.

Though Meder had very well characterized this substance, though Agaphi had ascertained the nature of the place in which it occurs, and though Lowitz had proved by analysis that the oriental turquoise contains merely a trace of lime, and no phosphoric acid, Reuss has notwithstanding made it only a fossil, a petrified substance.

To avoid all confusion, I shall reserve for the stony turquoise the name of *calaite*, given it by Pliny. This essay, therefore, shall be divided into two chapters. In the first I shall treat of a hardened clay, coloured by an oxide of copper, or an arseniate of iron—a substance which must occupy a place in the *oryctognostic* system. In the other I shall give an account of the fossils which have been found changed into turquoises by the contact of the requisite substances.

Under these two points of view we must divide the authors who have treated of the turquoise.

*Authors who have treated of the Calaité or the Stony Turquoise.*

Tavernier, J. B. *Voyages en Turquie, en Perse et aux Indes*, à Paris, 1678. 4to.

Boccone, intorno le Turchine o Turquoise della nova rocca. Museo di Fisica. Observ. 43. p. 278.

Meder et Lowitz, *Notices employées par Reuss, Mineralogie* ii. th. b. iii. p. 511.

Agaphi, Dmitrie, *Etwas von der eigentlichen Beschaffenheit des Orientalischen Turkis*. See Pallas *Neueste Nordische Beyträge*. B. i. p. 261. n. xiii.

Brückman in *Crell's Annalen*, 1799. B. ii. p. 185—199.

Fischer, Gotthelf, sur la Turquoise Orientale. See Mem. de la Soc. Imper. des Naturalistes de Moscou. Vol. i. de la Seconde Edition, p. 140—149.

John, J. F. Experience et Analyse Chimique de la Turquoise; ibid. n. xviii. p. 131—139. Bemerkungen über den Türkis in s. Chem. Untersuchungen. B. i. n. xxv. p. 190—192. In Gehlen's Journal für die Chemie u. Physik, iii. 1. 93.

Blumenbach, in v. Moll's neuen Jahrb. der Berg. u. Hüttenkunde, ii. 275.

*Authors who have treated of the Turquoise vulgarly so called, or of the Turquoise Odontolite.*

Guy de la Brosse, sur la Nature et l'Utilité des Plantes. Paris, 1628, p. 421.

Mortimer, Cromwell, Remarks on the precious Stone called the Turquoise. Phil. Trans. No. 482 and 483, p. 429.

Reaumur, Observations sur les Mines des Turquoise du Royaume sur la Nature et la Maniere dont on lui donne la Couleur. Mem. de l'Acad. des Sciences de Paris, 1715. P. 174—202.

Lommer in der Abhandlungen einer Privatgesellschaft in Bohmen, ii. p. 112.—The author pretends that the turquoise is an artificial production.

Brückmann's Abhandlung von Edelsteinen, p. 329—341. 1 Forts. p. 246—247. 2 Forts. p. 247—248.

Cuvier, G. Extract d'un Ouvrage sur les Especies de Quadrupedes dont on a trouvé les Ossemens dans l'Interieur de la Terre, an. 9. 4. p. 6.

Emmerling's Mineralogie, ii. p. 270.

Kirwan's Mineralogy, ii. 190.

Reuss's Mineralogie, ii. 3, p. 511.

Haüy's Traité de Mineralogie, iii. 570.

Brochant, Traité Element. de Mineralogie, ii. 212.

Suckow's Mineralogie, ii. 227.

Patrin, sur la Turquoise, Dict. et Hist. Nat. Art. Turquoise.

Bouillon la Grange, Ann. de Chimie, lix. 180.

Klaproth and Wolf, Dict. de Chimie. Art. Turquoise.

It is unnecessary to say here that the artificial turquoise, or the imitation of it by a paste, cannot enter into this dissertation. I shall have an opportunity of showing that all the *turquoise odontolites* have undergone a change of some kind or other by the action of fire, and in this point of view ought to be considered as artificial, at least in part.

The name turquoise seems to be owing to this, that those from Turkey were first known.

The object of this essay on the turquoise, of which I have already communicated the principal ideas to the Imperial Society of Naturalists, who have printed them in their Memoirs, and the principal interest of which depends upon the analysis of my esteemed friend Dr. John, is to assign the calaité a place in the



oryctognostic system, and to add to the notions which we have respecting the turquoise odontolite, some new discoveries, at the same time that I exclude it, as ought to be done, from the number of stony bodies.

### CHAP. I.—Of the Calaité.

#### *Description of the external Characters of the Calaité.*

*Calais*, Plin. *Бирюза* in Russian, *birousa* in Persian, *turquoise* vulgarly.

#### *Colour.*

Calaité is *blue*, intermediate between sky-blue and pale verdegri green; that is to say, of a peculiar blue, which must be called calaité, or turquoise blue. It may be obtained by mixing two parts of mountain blue with one part of mountain green.

This blue passes on the one side through smalt blue to the finest sky blue; on the other side through pistachio-green to apple-green, which does not yield in any thing to the most beautiful chrysoprase.

Yellowish-green and celadon-green are the colours of pieces altered by the atmosphere without being decomposed. Botryoidal portions are usually observed on the surface, sometimes surrounded by a layer of yellow matter down to their roots, giving to pieces thus cut the aspect of annular.

#### *External Shape.*

It occurs *massive*, in *layers*, and *disseminated*.

*a.* In reniform masses which, at the surface, are mameled and botryoidal; from the size of a nut to that of a goose egg. The largest piece that I have seen is in the museum of the Imperial University of Moscow, coming from the rich donation of his Excellency Councillor of State, the Chevalier Paul de Demidoff. This piece is  $3\frac{1}{2}$  inches long, 1 inch 8 lines in breadth, and 1 inch 2 lines in thickness in some places.\* It weighs four ounces five drachms. His Excellency Dr. Crichton, Councillor of State at Petersburg, possesses a piece which is not much smaller. This gentleman, equally celebrated and venerable for his medical skill and his goodness of heart, has formed a collection of minerals which may be called the coquetry of science. The rarest objects, the most perfect and most varied crystallizations, form the principal object of this collection. The third piece in point of grandeur belongs to M. Wenck. It weighs  $17\frac{1}{4}$  solotniks, or 1035 gr.

*b.* In rounded pieces; very rarely; I have seen in the possession of M. Wagner, member of our Society, a single piece, which seems to have been rounded by the action of water. I

\* This piece, like all those which pass legitimately in commerce, has the Persian mark of its origin and authenticity.



have since procured another, which, although a little altered at the surface, appears to have undergone the same change.

c. In layers, and disseminated in an umber brown substance, porous, and very hard, which I formerly took for a clay porphyry: but which I have more lately ascertained to be an indurated clay ironstone (*verhärteter Thoneisenstein*). Meder called it a clay slate, reposing on veins of quartz; but the matrix in which that variety of calaites is found, when treated by the blow-pipe, is attracted by the magnet, which leaves no doubt about its nature.

d. The rarest position of calaites in beds is in conchoidal siliceous schistus (Lydian stone), in which we find likewise very distinctly veins of quartz, but other veins are filled with layers of calaites. A very interesting piece which serves as a proof of this assertion may be seen in the fine collection of M. Wagner.

#### *Lustre.*

It is dull internally; of a waxy lustre in some pieces of a sky blue colour; *splendent* in those which are intimately combined with quartz.

#### *Fracture*

The fracture is *compact* or *subconchoidal* in the mamellated pieces; *conchoidal* in the blue, when the calaites occur in layers; in other specimens the fracture is *uneven* and *rough*, especially in some green varieties; in others, *fine scaly*; namely, in the quartz or vitreous calaites, especially in that which is formed in the siliceous schistus when the veins of quartz are not completely converted into calaites.

#### *Fragments.*

The fragments are indeterminate, often triangular with sharp edges.

#### *Transparency.*

It is commonly opaque, very rarely a little transparent on the edges.

#### *Hardness.*

It is *hard*, but not so much so as quartz, on which the sharp fragments make some scratches, but are speedily blunted, leaving a white powder. This is a very good way of distinguishing calaites from malachite, muriate of copper, or scoriaceous copper ore, which in some varieties approach a good deal to the blue or the green of calaites, so that the Boukhares often sell them for calaites.

Calaites yields with difficulty to the knife, and gives a white powder; the ores of copper, malachite, muriate of copper, &c. yield easily to the knife, and give a green powder, little different in colour from the mineral itself.

The whitish decomposed pieces are friable, adhere strongly to the tongue, and resemble exactly porcelain clay, sometimes snow white, or having a slight bluish tint,



*Physical Characters.*

It is moderately heavy. The specific gravity varies according to the different varieties :

Grass-green calaite . . . . .	2.7568	Pansner
Apple-green calaite. . . . .	2.6296	Ditto
Mamellated ditto . . . . .	2.860	Fischer
————— . . . . .	3.000	John
Slaty ditto . . . . .	3.250	Fischer

None of the varieties of calaite appears to acquire any electricity by friction.

*Chemical Characters.*

All the varieties of calaite remain unaltered though plunged into muriatic acid.\*

Muriate, or scoriaceous copper, which approaches much to some varieties of calaite, acquires, when plunged into the same acid, a more beautiful colour, and becomes transparent like the *emerald* ; but when dried, becomes covered with a white coat.

This examination of the external characters of several calaites shows clearly that there are three distinct species differing in their *fracture, colour, specific gravity, constituents, and position.*

1. *Calaite, properly so called.*

*Calaite*, Fischer, Mem. des Nat. i. p. 149. Onomasticon (1815) p. 8. familia Argillæ.

*Turcosa*, Fischer, Onomast. (1811) p. 53, after the wavellite. (Syn. *Turchesia* ; *Turchin*.)

*Türkis*, Ullmann, Mineral. einf. Fossilien, p. 76, n. 103.

*Dichter Hydrargillite*, Haussmann Handb. der Mineralogie, p. 444, c.

This species is almost always of the fine blue, which I have called calaite blue ; it occurs in reniform and botryoidal pieces ; it is opaque, and not even translucent on the edges. Sp. gr. 2.860, Fischer.

*Chemical Characters.*

Calaite is a clay, coloured by oxide of copper. Professor John made an interesting analysis of this variety for the Society of Naturalists. The museum of Moscow furnished him with the necessary specimens, with the permission of Chevalier Paul de Demidoff, as the collection was in possession of several. As it is interesting to know the process of M. John, I shall transcribe his account of it such as he deposited it in the archives of our Society.

\* The French jewellers consider it as a principle, that the true turquoise should effervesce in sulphuric acid. This is a proof that they think only of the French turquoise, or turquoise odontolite, the true stone, or calaite, not yielding to the strongest acids.

*a.* Two hundred parts of the mineral in fine powder were mixed with 10 times their weight of nitric acid, and subjected to ebullition for an hour. The mixture diluted with water and filtered left a brownish-grey powder on the filter. After washing and drying it, I put it aside for further experiments.

*b.* The nitric acid solution being evaporated to dryness, and the residue redissolved in water, left about one part of silica. The solution was divided into two parts.

*c.* A polished plate of iron plunged into one of these parts precipitated in a dendritical form  $3\frac{1}{2}$  gr. of copper.

*d.* The solution freed from copper was boiled with an excess of caustic potash. After having washed and dried the resulting precipitate, I obtained oxide of iron which contained a trace of alumina.

*e.* The liquid remaining from *d* having been saturated with nitric acid was decomposed by carbonate of ammonia. The earth precipitated in this way was separated from the liquid by filtration. Being redissolved in sulphuric acid, and mixed with a little potash by evaporation and crystallization, pure alum was obtained.

*f.* After having boiled the other half of the solution with caustic potash, a dark-brown precipitate fell, which was washed and dried, and digested for an hour in nitric acid. There remained one grain of oxide of iron.

*g.* After saturating the blue liquid thus freed from iron with ammonia, M. John added to it prussiate of potash. A brick-red precipitate fell, which, being washed, dried, and calcined, weighed  $4\frac{1}{2}$  gr. and consisted of oxide of copper. If we subtract the small quantity of iron shown to exist in this precipitate by prussiate of potash, the true weight of the oxide of copper will be  $4\frac{1}{4}$ , corresponding to the  $3\frac{1}{2}$  of copper above-mentioned.

*h.* The liquid freed from copper was neither altered by oxalic acid nor by the carbonate of potash.

*i.* The alkaline lixivium *f* was saturated with nitric acid, and decomposed by carbonate of ammonia. The earth precipitated in this way was separated from the liquid by the filter. After being washed and calcined, it weighed 70 gr. and was alumina.

*k.* The residue remaining from *a* was boiled with caustic potash. The mixture being diluted with water, then dissolved in nitromuriatic acid, evaporated to dryness, and redissolved in water, left a powder, which, being collected on the filter, washed, and calcined, weighed  $1\frac{1}{2}$  gr. It was silica proceeding from the pulverisation of the turquoise in the mortar.

*l.* On adding caustic ammonia to the nitromuriatic acid solution, a gelatinous precipitate fell, which, being collected on the filter and washed, was boiled with caustic potash. In that way three grains of oxide of iron were obtained.

*m.* The alkaline ley being saturated with an acid and decom-

posed by carbonate of ammonia, three grains of pure alumina were obtained.

*n.* The liquid *l* freed from alumina and iron was saturated with nitric acid, and mixed with a solution of prussiate of potash. By this means  $\frac{1}{4}$ th of a grain of oxide of copper was obtained. Thus 100 parts of the calaite subjected to experiment furnished

Alumina. ....	of <i>c</i>	70.00	}	73.0
	<i>m</i>	3.00		
Oxide of copper. ....	of <i>g</i>	4.25	}	4.5
	<i>n</i>	0.25		
Water .....				18.0
Oxide of iron. ....	of <i>f</i>	1.00	}	4.0
	<i>l</i>	3.00		
Lead and loss .....				0.5
				<hr/> 100.0

#### Position.

Calaite occurs in alluvial grounds, and as far as we know at present, only in the neighbourhood of Nichabour, in the Khorasan, in Persia. It ought to be of the old rock; for we find it very seldom in commerce, and I have only seen the specimens of it which I have mentioned above. It is probable that these kidney-shaped pieces occur in beds of a brownish clay. The rounded or rolled pieces necessarily belong to this species.

#### 2. Agaphite.

*Conchoidal calaite, conchoidal turquoise.* Mem. de la Soc. Imper. des Natur. i. 149.

The agaphite varies most in colour. It occurs of the palest and of the deepest sky blue. But its external figure is constant, as it occurs always in layers in an argillaceous oxide of iron, more or less hard. Its layers vary in thickness from a line and less to five lines. It is opaque; but the darkest coloured specimens, which are also the smallest, are translucent on the edges. Sp. gr. 3.25, Fischer; 3.00, John.

#### Chemical Characters.

We have not yet obtained an exact chemical analysis of this species; but we have no reason to doubt the assertion of Dr. Macmichael, who, just after his arrival from Sweden, assured us that the celebrated Gahn had undertaken an analysis of the *turquoise*, according to whom it is coloured by *arseniate of iron*. This analysis can only be applied to the agaphite.

#### Position.

It is found in beds accompanied by a very indurated argillaceous ironstone. The matrix has been sometimes called *tile-ore*,

sometimes *indurated clay*, sometimes *porphyry*, and sometimes *lava*. But having shown above that the matrix, when treated by the blow-pipe, becomes attractable by the magnet, an experiment which Dr. Macmichael repeated before my eyes, there can be no doubt that it is an argillaceous iron ore. It was respecting this species that M. Agaphi made researches on the spot without being intimidated by the fear of death, or of becoming a slave. Naturalists will doubtless concur with me in my desire to erect a monument, though not a very durable one, for such heroic researches, by giving to this species the name of *agaphite*. The following is the account which he sent to the late M. de Lawadowsky, Minister of Public Instruction, such as it was published by Pallas, and afterwards in our Memoirs, in the place cited above, among the bibliographical notes respecting calaité.

“On my return from India to Russia by land, I passed through the Khorasan, not far from *Pichapour* (*Nichabour*). I was informed, to my great satisfaction, that it was the only part in all Asia which possessed mines of turquoises. Eager to see these mines, I despised the risk of being made a slave, according to the custom of the country, and I resolved to examine myself the manner of obtaining the turquoise to remove my doubts, and thus to confer a benefit on the scientific world.

“The following are my observations on these mines. They satisfied myself, and will, perhaps, be agreeable to other naturalists. The mines of turquoise occur in mountains, which are not very high, and whose surface is covered with an arable soil mixed with sand, but which, in consequence of the heat of the climate, produces nothing but bent. No certain index of these precious stones occurs; but the inhabitants are led to suspect their existence from the ochre-brown pebbles which are found at the bottom of these mountains, and endeavour to discover them by digging pits of no great depth.

“I visited with much attention several mines already discovered, and I found that the matrix of the turquoise forms veins, which appear to extend in all directions as the branches of a single trunk, or as the secondary arms of a river; so that, when a small vein is discovered, it is only necessary to pursue it to discover others of more importance.

“The matrix of the turquoise occurs in horizontal beds (like that of chrysoprase) which have from 1 line to 10 lines in thickness. In these it is disseminated; so that a piece is very rarely found which is 12 or 14 inches in length and breadth. Among the beds which contain the turquoise, either in veins, or disseminated in grains, or reniform, are found likewise beds of the matrix of the same thickness, but without the turquoise.

“Among these veins are chosen the pieces, which contain the turquoises in mass, and very little of the matrix. It is difficult to discover among many pieces a pure turquoise of the size



of a pea. Those of the size of a nut are very rare, and very much valued, as the commerce of turquoises with the Afghans, the Persians, and other Asiatic nations, is very great."

The finest agaphite, or calaite, in layers which I have hitherto seen is in the collection of M. Weyer, jeweller at Moscow. It is of the finest colour, cut in the form of a heart, and is two inches five lines in length, and two inches nine lines in breadth at the broadest part. It is accompanied by the matrix, which has received the same form, to serve as a support to the stone, which of itself is too thin to be cut into a table shape. What renders this stone more remarkable is, that it served, according to report, as an amulet, or talisman, to Nadir Shah, containing a verse of the Koran very well engraved in gilt letters. It was purchased at Meched, and M. Weyer offers it for sale at the price of 5000 roubles.

### 3. *Johnite*.

Quartzy turquoise, vitreous, or scaly. Mem. de la Soc. des Nat. i. 149.

It has a light-blue colour, which passes into green. It occurs in very thin layers, in a black siliceous slate. It is harder than the two other species, scratches glass strongly; but does not give sparks with steel. The fracture of it is scaly.

This species, as I mentioned before, is more rare than the others. I am acquainted only with one specimen in the collection of M. Wagner. It certainly exists more frequently in nature, but is seldom met with in commerce, because it is not fit to be polished.

Its specific gravity and chemical composition are unknown; but it is probable that it contains some silica, in consequence of the siliceous matrix with which it is accompanied. We have not been able to discover any thing respecting its position.

I have given to this species the name of Johnite, in honour of Prof. John, of Berlin, who, by his chemical researches, daily gains more and more of the esteem of men of science.

### *Uses.*

Calaite is employed as an ornament in diadems, bracelets, rings, with or without brilliants; or, especially among the Persians, to adorn the handles of knives, sabres, &c. or to construct talismans, as I have mentioned above.

### CHAP. II.—*Of the Odontolite, or Occidental Turquoise.*

The article respecting the odontolite turquoise requires to be treated as an object of zoognosy. The following are the principal questions which require to be answered:

1. What are the parts of the skeleton hitherto found converted into turquoise?
2. To what animal do they belong?
3. Where do the principal depots occur?



*Answer to the first Question.*

If we give the name of turquoise to every animal substance which has been penetrated and coloured green or blue by metallic oxides, and particularly by copper, it is obvious that any part of the skeleton, and even the whole body, may have been converted into turquoise provided all the parts be capable of undergoing the change. But it appears that the teeth are the only parts which possess sufficient hardness to become true turquoises in the full acceptation of the word. If entire skeletons,\* or parts of skeletons, still surrounded with dried muscles, have appeared to assume the form of turquoise, it seems more reasonable to consider them as passages to that state than as true turquoises.

There can be no doubt that Bouillon la Grange analyzed a French turquoise, or a bone turquoise. He found its specific gravity 3.127. Before the blow-pipe it became greyish-white without melting. This operation rendered it friable, and it lost 0.06 of its weight. Its solution in nitric and muriatic acids was colourless. It was composed of

Phosphate of lime . . . . .	80
Carbonate of lime . . . . .	8
Phosphate of iron . . . . .	2
Phosphate of magnesia . . . . .	2
	<hr/>
	92

The experiments of Prof. John with the blow-pipe, in presence of the members of the Imperial Society of Naturalists, in order to change the teeth of the mammoth into turquoises, appear to contradict those of Bouillon Lagrange; but if we consider that the turquoises of *Simore* have already undergone a degree of calcination, it is not surprising that they appear grey before the blow-pipe.

*Answer to the second Question.*

Naturalists have hitherto spoken only of two animals whose teeth are capable of furnishing turquoises. These are those which Reaumur has described, and of the teeth of which he has given figures.

1. *Dentes Molares, with four Eminences of considerable Size.*—These teeth appear to belong to an animal similar to that of the

\* Swedenburg has engraven the figure of the skeleton of a quadruped which had been coloured by this metal. We see in the Museum d'Hist. Nat. of Paris the hand of a woman, the extremities of the fingers of which are green, and the muscles of which, dried, like a mummy, are also green. If it has been said that the whole of this hand has been converted into turquoise, the fact has been exaggerated, and the term turquoise abused. But the exaggeration is true if we give the name of turquoise to an animal substance penetrated or coloured by an oxide of copper.

Ohio, or the carnivorous elephant. It is the *mastodonte* of Cuvier, and the *mastotherium* of my Zoognosy.\*

The upper part of that which Jussieu has figured, and which Reaumur reports, pl. 7, fig. 17, was five inches in diameter, and five inches long, although the roots were not complete.

2. *Teeth with four to five obtuse Eminences, and less elevated.*—Reaumur, pl. 7, fig. 1, 3.

These teeth, with tubercles, of the summit more obtuse, and a little channelled, present naturalists with two species very different in size belonging to a new genus of fossil animal.

I have observed that property in the teeth of other species of animals, and I here give the description and the figures.

3. *Dens Molaris, with a flat Summit, and Plates turned upon themselves, with two principal Folds, which almost touch the external Surface.*—(See Plate XCIX, fig. 1, reduced one-fifth.)

This tooth belongs to an animal unknown to zoologists. It was completely penetrated with the green colour, so that it had the appearance of being composed of malachite. This tooth was given to our Society by M. Nikite de Mouraview, but it was unfortunately destroyed in 1812 by the flames.

4. *Dens Molaris, elongated with a flat Summit, with Plates turned on themselves, and two Folds less deep and equally distant from the external Surface.*—(See fig. 2, reduced in the same proportion.)

We do not know the animal to which this tooth belongs. It presents a slight curvature, which in others is greater. I have seen some of them green, some azure blue, and others only partially coloured.

Native place, *Siberia, Miask.*

5. *Dens Molaris, with a flat Summit, and Plates triply folded, so that each Fold encloses one or two compressed Tubes formed by a vitreous Substance.*—(See fig. 5.)

A singular character belonging to this tooth is to possess in the principal channel a kind of stalactite of vitreous matter (fig. 5, 6, \*,) which I have observed in all similar teeth.

The animal which possessed teeth of this kind is unknown to naturalists.

I likewise lost this tooth by the flames, but there is a similar one of a very deep-green colour in the rich museum of the Imperial Academy of Sciences of St. Petersburg. One of my pupils gave me a third of the same animal, but it has only a slight tinge of azure blue.

I have reason to believe that these teeth come from Miask, in Siberia.

\* The idea of giving to all the fossil mammalia the same termination, which Cuvier has applied to different animals, as *megatherium*, *anoplotherium*, *palæotherium*, is at least very useful for communicating information. It has induced me to change Jefferson's *megalonix* into *onychotherium*, and to form the words *elasmotherium*, *trogontherium*, *pterotherium*, though I am not ignorant of Linnaeus's rule which excludes such generic names. But it can no longer be followed, at least as far as the great series of fossil animals without vertebræ is concerned.

6. *Dens Molaris* of an *Animal* of the *Stag Genus*.—(See fig. 6.)

This tooth was found in a copper mine in the government of Olonez, which has been abandoned these 20 years. I am indebted for it to the kindness of M. de Foullon, who possesses, perhaps, the most instructive collection of these interesting countries.

7. *Dens Molaris* of a *carnivorous Animal*.—(See figs. 3 and 4.)

This tooth has lost one of its roots. The eminences of the summit are partly obliterated. It is entirely covered with a verdigris-coloured oxide of copper.

The place where it was found is unknown. It is very probable that we shall hereafter discover several other teeth equally entitled to the name of turquoise. And from what I have said in answer to the first question, other parts may be susceptible of the same change.

*Answer to the third Question, Where do the principal Depots occur?*

The principal depots which have served to form turquoises are those of France. They occur in Lower Languedoc, near the town of *Simore* and its environs, as at *Baillabatz* and at *Laymont*. There are others, according to Reaumur, nearly in the same country, on the side of *Auch*, at *Gimont*, and at *Castres*.

Guy la Brosse, in his work (of 1628, p. 421), On the Nature and Utility of Plants, is the first person that makes mention of it. He calls *Licorne minerale* and mother of the turquoises (p. 467 and 521). "The licorne," says he, "is a stone having the shape of a horn, and the consistence of a stone, which, being exposed to a graduated heat, gives the true turquoise. It is called *licorne minerale* because it resembles the horn of an animal.

We learn two things from this notice of Guy la Brosse :  
1. That tusks were employed for forming good turquoises ;  
2. That the true colour is given by the fire.

Reaumur described afterwards the manner of heating the turquoise, and gives a figure of the peculiar furnace employed for the purpose. From this, what I stated above follows clearly ; that the French turquoises are prepared by the fire, and consequently are partly artificial.

Other depots are likewise mentioned, from which I have not yet seen specimens ; namely, *Nivernois*, *Silesia*, *Lesta* in *Bohemia*, *Thurgau* in *Switzerland*. We must add likewise from my observations, *Siberia* (*Miask*), and the government of *Olonez*. The number of places will doubtless augment in proportion as more attention is paid to fossils.

I shall finish this dissertation with the question, how can we distinguish a turquoise from a calaite, or the turquoise of France from the turquoise of Persia ? The hardness is the first character. Distilled vinegar deprives it of its colour, and nitric acid dissolves

it completely. The polish of the turquoise is not so good as that of the calaite, and exhibits plates, rays, or filaments, depending upon the bony structure. Rosnel affirms with justice that all the turquoises of France have their surface covered with radii, or filaments. And though Reaumur affirms that the more sensible the plates are, the worse are the turquoises, this circumstance does not fail to be a certain character for distinguishing the tooth turquoises of France from the true mineral, or the calaite of Persia.

### ARTICLE III.

*Researches on a new Mineral Body found in the Sulphur extracted from Pyrites at Fahlun.* By J. Berzelius.

(Continued from p. 265.)

3. *Seleniate of Ammonia*.—We obtain the neutral salt by dissolving selenic acid in concentrated caustic ammonia, leaving a small excess of acid. We leave the liquid in a temperate place; the seleniate is gradually deposited in crystals, partly feather-shaped, and partly in four-sided prisms, or oblique four-sided prisms. The crystals deliquesce in the air.

The *biseleniate* is formed when the neutral salt is dissolved in water, and the solution allowed to evaporate spontaneously. A portion of the ammonia exhales with the water, and the biseleniate is deposited in acicular crystals which are not altered by exposure to the air.

The *quadriseleniate* is produced by the evaporation of a solution of the biseleniate by means of heat, or by the addition of selenic acid to the biseleniate. It does not crystallize, and, when evaporated to dryness, soon recovers its liquidity by absorbing moisture from the atmosphere.

If we heat the seleniate of ammonia in a retort, water and ammonia are at first disengaged; then the ammonia decomposes the selenic acid, producing azote and water which distils over. This is usually followed by a little quadriseleniate, which is likewise sometimes deposited in a dry state on the upper part of the retort. At the bottom we find a button of melted selenium. The decomposition takes place with a strong effervescence, but without detonation, at least with the small quantities on which I operated.

4. *Seleniate of Barytes*.—The neutral salt is insoluble in water, pulverulent, and does not alter the colour of moist litmus paper. At the temperature of melting glass it does not melt, and it does not appear to contain combined water. It is soluble both in selenic acid and in the strong acids. We obtain this



salt by precipitating muriate of barytes with a neutral alkaline seleniate.

The *biseleniate* is formed when the carbonate of barytes is dissolved in selenic acid, till all effervescence is at an end. When the solution is evaporated, the biseleniate crystallizes in the form of round grains, which are transparent, and have sometimes a smooth surface. On breaking these grains, we find them composed of concentric rays. If the liquid contains no excess of acid but that which is in the biseleniate, and if we allow it to evaporate spontaneously, it deposits a milk-white mass, composed of small opaque grains. This mass dissolves slowly in water, and appears at first little soluble; but when once dissolved, it remains in solution even when a considerable portion of the water is evaporated. A solution of biseleniate mixed with ammonia lets fall the neutral seleniate.

Two grammes of the neutral seleniate of barytes which had been dried in a red heat, having been dissolved in muriatic acid, left a little selenium, which was made to disappear by the addition of nitric acid. The barytes was then precipitated by means of sulphuric acid. The sulphate of barytes, after being well dried, weighed 1.765 gramme, equivalent to 1.1586 gramme of barytes. Hence 100 parts of selenic acid had been combined with 137.7 parts of barytes, which contain 14.32 of oxygen. This agrees very well with the capacity of saturation of the acid given above.

1.271 gramme of biseleniate of barytes, deprived of all moisture, yielded 0.785 gr. of sulphate of barytes, equivalent to 0.5153 of barytes. Hence 100 parts of selenic acid had been combined with 68 parts of barytes, or about the half of the base contained in the seleniate. This shows that the same relation exists between these salts as between those which have a base of soda.

5. *Seleniate of Strontian*.—The neutral salt is a white insoluble powder. The *biseleniate* may be obtained by dissolving the carbonate of strontian in selenic acid. When the solution is evaporated slowly, the salt is deposited in milk-white crusts, which exhibit no trace of crystallization, and which dissolve again with great difficulty even in boiling water. The dry biseleniate, when heated, melts at first, and gives out its water, swelling at the same time, and forming a porous mass, from which the heat drives off the excess of acid. The neutral seleniate which remains does not melt.

6. *Seleniate of Lime*.—The neutral salt is but little soluble in water. It precipitates gradually as we dissolve carbonate of lime in selenic acid. When dried, it forms a crystalline powder, soft to the touch, and precisely similar to carbonate of lime. When heated to redness, it liquefies. The melted salt attacks the glass, a kind of effervescence takes place, by which the substance of the glass, and not the melted salt, is filled with small bubbles which dilate, and at last perforate the glass, so



that the melted salt runs out. This property, which the seleniate of lime has in common with the seleniates of magnesia and of manganese, is very remarkable. I do not pretend to explain it.

The *biseleniate* is obtained when the preceding salt is dissolved in selenic acid. The solution crystallizes to the last drop in the form of small prisms, which are not altered by exposure to the air. Caustic ammonia removes one half of the acid, and leaves the neutral salt. Heat produces the same effect.

7. *Seleniate of Magnesia*.—Selenic acid decomposes the carbonate of magnesia with effervescence without however dissolving it, because the new compound is very little soluble. The result is a crystalline powder. It is soluble in boiling water, but a great quantity of that liquid is requisite. The solution when evaporated gives small crystalline grains, which under the microscope exhibit the form of small prisms, and small four-sided tables. When heated, the salt gives out its water of combination, and becomes enamel-white. Heat does not melt it, nor drive off its acid; but it attacks the glass vessel, which swells up, and is penetrated by a great number of bubbles.

We obtain the *biseleniate* by dissolving the preceding salt in selenic acid and adding alcohol to the solution. The biseleniate precipitates in a pulpy coherent mass, which attracts moisture from the air, and which does not crystallize though it be dissolved in water and the solution evaporated.

8. *Seleniate of Alumina*.—The neutral salt is insoluble. We obtain it by evaporating a solution of muriate of alumina to dryness, dissolving the salt in water, and precipitating it by biseleniate of ammonia. The solution of alum is not precipitated; but if we mix with it a neutral seleniate with an alkaline base, the seleniate of alumina is precipitated. The precipitate is a white powder, which, when heated, gives out in the first place water, and afterwards the whole of its acid.

The *biseleniate* is produced by dissolving the preceding salt, or the hydrate of alumina in selenic acid. The liquid has an astringent taste, and, when evaporated, yields a colourless, transparent mass, similar to gum.

9. *Seleniate of Glucina*.—The neutral salt is a white insoluble powder. The *biseleniate* is soluble. When evaporated, it leaves a mass resembling gum. Both salts lose their acid when heated.

10. *Seleniate of Yttria*.—The seleniates with an alkaline base, when dropped into a solution of yttria, throw down large white flocks, which do not dissolve in an excess of selenic acid. When dried, the salt has the form of a white powder, which, when heated, lets go first its water, and then its acid.

11. *Seleniate of Zirconia*.—This salt is a white powder, insoluble both in water and in selenic acid. Heat decomposes it.

12. *Seleniate of Zinc*.—The neutral salt is a crystalline powder, insoluble in water. When heated, it first gives out its water of

crystallization, and then melts. The fused mass is yellow and transparent; but on cooling, it becomes white. Its fracture is crystalline. When exposed to nearly a white heat, it boils, and a part of the acid is disengaged. At last the mass becomes solid. It is then in the state of a subseleniate of zinc no longer altered by heat.

The *biseleniate* is very soluble in water. Its solution when evaporated gives a transparent mass, full of small rents, and similar to gum.

13. *Seleniate of Manganese*.—The *neutral salt* is a white insoluble powder, which, when dried, forms a soft meal, like the seleniate and carbonate of lime. It is very fusible, but preserves its acid well in close vessels; but if the air have free access, the manganese unites with more oxygen, and the selenic acid is disengaged. The seleniate of manganese in a state of fusion possesses the property of destroying glass in a much higher degree than the seleniates of lime and magnesia. The bubbles which form in the glass are larger, they come to the surface, and opening, leave holes. The interstices between these holes are not liquefied, and the glass thus decomposed is not at all coloured by the oxide of manganese.

The *biseleniate* is very soluble. When evaporated, it gives a crystallizable saline mass. A high temperature reduces it to the neutral state.

14. *Perseleniate of Uranium*.—The neutral salt is a lemon-yellow powder, which, when heated, gives out its salt and a portion of oxygen, leaving green oxide. *Biperseleniate* is formed by the solution of the preceding salt in selenic acid. By evaporation, a varnish of a pale-yellow colour, and transparent, is obtained. When entirely dry, it is white, opaque, and crystalline.

15. *Perseleniate of Cerium*.—Both the neutral salt and the biperseleniate resemble exactly the same salts of the peroxide of uranium.

16. *Protoseleniate of Cerium*.—A white insoluble powder, which dissolves in selenic acid, and forms a soluble biseleniate. This is one of the few properties by which the protoxide of cerium differs from yttria.

17. *Protoseleniate of Iron*.—Selenic acid hardly attacks iron. The metal assumes a copper colour, being covered by a thin coating of selenium, and this puts an end to all action.

If we mix a protosalt of iron with a neutral alkaline seleniate, a white precipitate falls, which gradually assumes a grey colour, and then a yellow, in proportion as the air acts upon it. After being separated, washed, and dried, it is yellowish-white. If we pour muriatic acid on seleniate of iron newly formed, and especially if we apply a little heat, the acid is decomposed, leaving selenium reduced, and not dissolved. By the action of the acid, the protoxide of iron reduces a part of the selenic acid,

and the muriatic acid dissolves the peroxide of iron with a portion of the selenic acid not decomposed. This is the reason why the muriatic solution assumes a yellow colour.

The *biseleniate* is obtained when the preceding salt is dissolved in selenic acid, or when we mix a protosalt of iron with a soluble biseleniate. The biseleniate of iron is but little soluble, and begins very speedily to be decomposed. If we heat a solution containing this biseleniate, it is decomposed, and yields a brown precipitate. It is a mixture of perseleniate and reduced selenium.

18. *Perseleniate of Iron*.—The neutral salt precipitates by double decomposition: a white powder falls, which becomes a little yellowish on drying. The perseleniate when heated gives out first its water of combination, and becomes red. At a higher temperature the acid sublimes, and may be entirely driven off.

If we dissolve iron in a boiling hot mixture of selenic acid and nitromuriatic acid, taking care that all the nitric acid is not decomposed, the liquid deposits during its cooling on the sides of the vessel a pistachio green salt in leaf-form crystals. I have reasons for considering it as a biperseleniate of iron. It does not dissolve in water, but muriatic acid dissolves it, and assumes an orange colour. Caustic potash added in excess gives a red precipitate; from which it follows that the green colour is not owing to the presence of protoxide of iron. When this salt is exposed to a high temperature, it gives out its water of crystallization, and appears black, but becomes colcothar-red on cooling. When the temperature is increased, selenic acid is disengaged without any trace of reduced selenium, which would not have been the case if it had contained protoxide of iron. Nothing remains ultimately but red oxide of iron.

If we digest either of the above-described seleniates with caustic ammonia, that alkali separates a portion of the selenic acid, and there remains a red subperseleniate, which, like the other subsalts of peroxide of iron, passes through the filter when we attempt to wash it. This subsalt is decomposed by heat, and leaves pure peroxide. From an analytical experiment to which, however, I do not attach much confidence, this subsalt is composed of

Acid. ....	52
Peroxide. ....	48
	<hr/>
	100

and the acid and base contain equal weights of oxygen.

19. *Seleniate of Cobalt*.—The neutral salt is a rose-coloured insoluble powder. The *biseleniate* gives, when evaporated, a beautiful shining-red varnish.

20. *Seleniate of Nickel*.—The neutral salt while still moist is

a white insoluble powder, which, when dried, assumes a pale-apple-green colour. The biseleniate is soluble, and gives a green mass resembling gum.

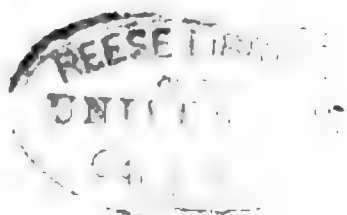
21. *Seleniate of Lead.*—Selenic acid precipitates the oxide of lead both from the muriate and the nitrate. The precipitate formed in the latter salt contains always nitric acid. We obtain pure seleniate of lead by mixing muriate of lead with an excess of seleniate of ammonia. A heavy white powder is formed, which speedily falls to the bottom, and is not redissolved by an excess of acid. The seleniate of lead melts like the muriate; but it seems to require rather a higher temperature. The melted mass is transparent and yellowish; on cooling, it recovers its whiteness, loses its transparency, and exhibits a crystallized fracture. In a reddish-white heat, the seleniate of lead begins to boil, and selenic acid sublimes. After some time the ebullition stops, and a melted subseleniate of lead remains, which, on cooling, is semitransparent and friable. Its fracture exhibits a very crystalline texture. Seleniate of lead still moist, though repeatedly digested in caustic ammonia, does not part with any acid, nor can it be converted in that way into subseleniate.

It is difficult to decompose seleniate of lead entirely by sulphuric acid. It is necessary that the acid be concentrated, and boiling. One hundred parts of seleniate of lead yielded 90.63 parts of sulphate of lead, equivalent to 66.67 parts of protoxide of lead; so that 100 parts of selenic acid combine with 200 parts of protoxide of lead, the oxygen of which is 14.342. This agrees with the results given above.

Two grammes of nitrate of lead dried to a fine powder, dissolved in water, and the solution then poured into an excess of seleniate of ammonia produced 2.01 grammes of seleniate of lead, dried in a temperature above  $212^{\circ}$ . Sulphuric acid being added to the liquid from which the seleniate had been precipitated separated 0.0075 gramme of sulphate of lead. This experiment gives likewise 200 of oxide of lead for 100 of acid. We see likewise that seleniate of lead is not absolutely insoluble in water.

22. *Protoseleniate of Copper.*—We obtain this salt in the form of an insoluble white powder, when protohydrate of copper is digested in selenic acid.

23. *Perseleniate of Copper.*—When we mix a hot solution of sulphate of copper with a solution of biseleniate of ammonia, a yellowish precipitate falls in very bulky flocks. This precipitate rapidly diminishes in volume, and in a few moments is converted into a mass of small silky crystals, of a very brilliant greenish-blue colour. These crystals are a neutral seleniate. The change of the flocks into crystals seems to be merely a change of aggregation occasioned by heat. The same thing takes place, but more slowly, if we mix the ingredients cold. Perseleniate of copper is neither soluble in water nor in selenic acid. When





heated, it gives out in the first place its water of crystallization, and becomes of a liver-brown colour. At a higher temperature, it melts, and becomes black. It then begins to boil, gives out its acid, and leaves at last only solid oxide of copper.

The *subperseleniate of copper* is a pistachio-coloured insoluble powder, obtained by precipitating persulphate of copper by a seleniate of ammonia with excess of base. It is soluble in an excess of ammonia. When heated, it becomes black, and gives out its water; it then swells up, and loses its acid.

24. *Perseleniate of Tin*.—This salt is a white powder insoluble in water, but soluble in concentrated muriatic acid. Water precipitates it from that solution. It is decomposed by heat, giving out first its water, then its acid, and the oxide of tin remains. I have not examined the combination of protoxide of tin with selenic acid. It is probable that it partakes of the properties of the protoseleniates of iron and mercury, and that it reduces a portion of its acid by the influence of a stronger acid, or even by heat, in order to form a higher degree of oxidation.

25. *Protoseleniate of Mercury*.—Selenic acid precipitates the soluble salts containing protoxide of mercury. The precipitate is a white powder, insoluble even in an excess of acid. When heated, it melts, and forms a mass of a very deep-brown colour. The colour diminishes as the matter cools, and the mass when solid has a lemon-yellow colour. If we raise the temperature a little higher, the salt begins to boil, and distils over in brown drops, which, on cooling, consolidate into an amber-coloured mass, most frequently transparent. Caustic potash decomposes this salt, separates the acid, and leaves the oxide in the state of a black powder. Muriatic acid decomposes it also, even when it has been melted; it dissolves the oxide of mercury with a little of the selenic acid, and leaves selenium reduced, just as I described the phenomenon when speaking of the seleniuret of mercury.

26. *Perseleniate of Mercury*.—The neutral salt obtained by saturating selenic acid with red oxide of mercury, or procured by double decomposition, is a white insoluble powder, or at least very little soluble.

The *biperseleniate* is obtained when to selenic acid a sufficient quantity of peroxide of mercury is added to occasion a commencement of the formation of neutral seleniate. The liquid is filtered, and then evaporated till it is sufficiently concentrated to yield crystals. It produces very large prismatic crystals, striated longitudinally, which contain a great deal of water of combination. It is very little soluble in alcohol. The alkalies decompose it with difficulty; even caustic potash does not entirely separate the oxide of mercury. Ammonia and the alkaline carbonates occasion no precipitate in it. The biperseleniate of mercury has exactly the same taste as the corresponding



muriate. It is fusible in its water of crystallization, which evaporates by little and little. The anhydrous salt is not fusible, and sublimes without alteration when heated. A solution of biperseleniate of mercury, mixed with sulphurous acid, gives immediately a white or greyish precipitate of protoseleniate of mercury; but some moments after, this precipitate assumes a fine cinnabar colour, owing to a portion of reduced selenium, which is deposited so uniformly on the seleniate that we might say that it is combined with it.

The red oxide of mercury digested with selenic acid till the neutral perseleniate has begun to form, is always intimately mixed with this perseleniate, which renders its colour more pale. This oxide produces, at first view, the singular phenomenon of evolving at once oxygen gas and a sublimate of protoseleniate of mercury. The reason is, that the uncombined oxide is decomposed by the heat, and the reduced mercury enters into combination with the perseleniate, and converts it into protoseleniate, which, at that temperature, is volatile, and sublimes.

27. *Seleniate of Silver*.—Selenic acid precipitates a solution of nitrate of silver. The white precipitate is a neutral seleniate. It dissolves in small quantity in boiling water. Boiling nitric acid dissolves it entirely, but it precipitates again when cold water is added to the solution. If we mix a boiling-hot nitric solution of this salt with boiling water, and allow the mixture to cool slowly, the seleniate of silver forms small acicular crystals. Light does not blacken it. The seleniate melts almost at the same temperature as the muriate of silver, and becomes transparent like it. When cooled, it forms a white, opaque, friable mass, having a crystalline texture. In a red heat, exposed to a current of air, it allows oxygen gas and selenic acid to escape, while it becomes covered with a pellicle of metallic silver.

2.687 grammes of seleniate of silver, which had been strongly heated, but not melted, were dissolved in boiling nitric acid, and the solution was poured in a solution of common salt. The resulting muriate of silver weighed 2.235 grammes, equivalent to 1.8082 gramme of oxide of silver. Therefore 100 parts of selenic acid had been combined with 205.75 parts of oxide of silver, the oxygen of which is 14.2. This agrees very well with the experiment described above.

(To be continued.)

## ARTICLE IV.

*Experiment with the Solar Microscope.* By Mr. James Watson.

(To Dr. Thomson.)

SIR,

London, Sept. 23, 1819.

A FEW years ago, while engaged in making some experiments on the prismatic colours, I was agreeably surprised by the sudden appearance of a new and interesting picture. In beauty it far excelled the common spectrum. It rather resembled a cloud, composed of the finest colours; and having a peculiar veined or clouded appearance, which added variety and richness to this exquisite picture. I have frequently repeated the experiments, with additional improvements, from time to time, to the great pleasure of several friends who have seen it. As some of your readers may wish to judge for themselves, I shall now beg to communicate the following particulars through the medium of your journal.

Any person having a solar microscope will only require in addition a glass prism and a pair of tin plate tubes, about two inches long, and about the same in diameter, each fitted with a convex lens at one end. These tubes may be found ready prepared in a middle sized magic lantern.

I shall now describe an easy and convenient method of mounting the prism and tubes. Procure two pieces of mahogany, one piece 18 inches long, 5 inches broad, and 1 inch thick; the other 4 inches square, and 2 inches thick. Take the long piece of mahogany, and at three inches from one end, on the flat side, cut a shallow hole, just large enough for one end of the glass prism to turn in. A piece of stout brass wire is then to be stuck into the wood, at two inches from the same end. If then this wire be bent over at the top into the form of a ring, it will support the upper end of the prism, and hold it in a perpendicular position, admitting also of its being turned round. This being finished, take some of the same brass wire, and make two rings with stems to them, each ring just large enough to receive one of the tin tubes. Then bore a couple of holes,  $2\frac{1}{2}$  inches apart, in the square piece of mahogany; place the stems of the wire rings in these holes, and see that the rings do not stand higher than the upper end of the prism. The tubes are now to be placed in the rings with the glasses towards each other; their distance can be adjusted by moving them backwards or forwards; and if the wires have room to turn in the holes, an horizontal motion may be given to the tubes when necessary.

The apparatus is now prepared for use. The best time for performing the experiment is from spring to autumn, about the hour of noon, and when the sky is free from clouds. The room is to be made as dark as possible, and the solar apparatus fixed

to the shutter in the usual manner, but omitting to screw on the Wilson's microscope. Instead also of placing the white paper screen opposite to the window, it must be placed sideways on the right hand side of the window. Now take the long piece of mahogany with the prism at one end, and place the tubes with their stand upon the other end. This apparatus is to be supported on a table close to the window in such a manner that the middle of the prism may be within a few inches of the end of the microscope, and the tubes within about 12 inches of the paper screen. The sun's rays on being admitted into the room will strike on the prism, and be divided. By a slight motion of the apparatus, the coloured rays may easily be made to fall on the inner surface of one of the tin tubes, from whence they will be reflected, and then refracted by the two convex glasses; after which, the picture already described, will make its appearance upon the screen.

The cloudy appearance arises from inequalities in the reflecting surface of the tin. This effect may be increased by making a few small indentations on the inner surface of the tubes. When the clouds are well defined on the screen, and the colours bright, then the apparatus is rightly adjusted. By altering the position of the tubes, or by using one, instead of both, some very pleasing changes will be produced.

I have been thus particular, in order that the operator may not fail to succeed. Perhaps this experiment will serve as a good illustration of that splendid display of nature's colouring which we sometimes witness about the time of sun-set, when the "shifting clouds in all their pomp attend his setting throne."

I am, Sir, your most obedient servant,

JAMES WATSON.

## ARTICLE V.

*Population of Bombay.\** By Sir James Macintosh, M.P.

THE public has hitherto received little authentic information respecting the population of tropical countries. The following documents may, therefore, be acceptable, as contributions towards our scanty stock of knowledge on a subject which is curious and not unimportant.

No. I. is an account of the deaths in the island of Bombay from the year 1801 to the year 1808 inclusive, founded on returns made to the police office of the number of bodies buried or burned in the island. These returns being made by native officers, subject to no very efficient check, may be considered as

\* From the Transactions of the Literary Society of Bombay.

liable to considerable errors of negligence and incorrectness, though exempt from those of intentional falsehood.

The average deaths during the year would, by this account, be 9000; but the year 1804, in which the deaths are nearly trebled, was a season of famine throughout the neighbouring provinces on the continent of India. Great multitudes sought refuge from death at Bombay; but many of them arrived in too exhausted a state to be saved by the utmost exertions of humanity and skill. This calamity began to affect the mortality in 1803, and its effects are visible in the deaths of 1805.

No. II. is an account of the Mussulman population; distinguishing the sexes, and conveying some information respecting their age, occupation, and domestic condition. This document and that which follows are the more important, because we have only conjectural estimates of the whole population of the island, which vary from 160,000 to 180,000 souls. By comparing the Mahometan deaths, on an average for the three years 1806, 1807, and 1808, as collected from No. I. with the whole number of Mahometans in this account, the deaths of the members of that sect appear to be to their whole numbers as 1 to  $17\frac{1}{4}$ .

No. III. is an account of the total number of Parsee inhabitants, distinguishing sexes and ages. From the same comparison as that stated in No. II. it appears that the deaths of the Parsees are nearly as 1 to 24.

Nos. IV. V. VI. and VII. contain accounts of population, births, and deaths, of native Christians, from four of the parishes into which the island is divided. Their baptismal registers furnish an account of the number of births, which we have no easy and precise mode of ascertaining among the other inhabitants. Their account of deaths is also some check on that part of the general register of deaths which relates to them; and their returns of the population are a further aid towards the formation of a general rate of mortality. In No. IV. the births are to the population as 1 to 28, the deaths as 1 to 20. In No. V. the births as 1 to 20, deaths as 1 to 16. In No. VI. births 1 to 30, deaths 1 to 15. In No. VII. births 1 to 43, deaths 1 to 22.

These proportions of births and deaths to population differ very considerably from each other, and some of them deviate widely from the result of the like inquiries in most other places. It is not easy to determine how far inaccuracy may have contributed to this deviation. The education of the native Roman Catholic clergy of Bombay is almost exclusively confined to monastic theology and ethics; even their respectable European superiors are fully occupied by their ecclesiastical duties, and are little accustomed to political arithmetic. On the other hand it must be remembered, that at Bombay, a population of 150,000 souls is confined to an island which is only eight miles in length and three miles in its utmost breadth. Such a population with



so limited a space must be considered rather as that of a town than of a district of country. It is to be expected, or at least not to be wondered at, that it should not maintain itself without the influx of inhabitants from the neighbouring provinces. The very small proportions of births in No. VII. probably arises, in part, from the number of adventurous strangers who resort to the most thickly peopled part of the island, while the three former returns, which relate to places where the Christians are native inhabitants, show a proportion of births by no means so singular. That the proportion of deaths in No. VII. is the least among the Christian returns, is in all likelihood to be ascribed to the easy circumstances of many of the members of that congregation, the Christians of the other parishes being chiefly of the very lowest classes. Of the high rate of mortality in Nos. V. and VI. which relate to two small fishing villages, no specious explanation presents itself: of that, and indeed of every other part of the subject, we must expect explanations from the enlightened and accomplished men on the spot, who now possess better means of investigation than were in such hands when these imperfect returns were procured.

It must be observed that many of the Parsees come to Bombay in search of fortune after having reached the age of manhood, and return with a competency to their native countries. Some of them are men of great wealth; many are in easy circumstances: and none are of the most indigent classes. From these circumstances, the comparatively low rate of their mortality and the smaller number of their females will be easily understood. The famine increased their mortality from 311 in 1802 to 563 in 1804; an augmentation almost entirely to be attributed to deaths of the fugitive Parsees, who were attracted to Bombay by the well-known charity of their opulent fellow-religionists.

The Mahometans are much inferior in fortune to the Parsees; but they are not much engaged in the lowest sorts of labour, which are chiefly performed by the inferior castes of Hindus, and by some of the native Christians. The famine increased the deaths of the Mahometans from 1099 in 1802 to 2645 in 1804.

Of the Hindus, who form the great body of the people, we have unfortunately no enumeration; but the return of their deaths has one observable peculiarity. In the higher castes the bodies are burned; in the lower they are buried. Though there be many individuals of the higher castes who occupy very humble stations, and are of what an European would call very low rank, there are scarcely any of the lowest castes in conditions of ease, not to say affluence: burning or burial affords, therefore, some criterion of their situation in life. The famine increased their mortality from 3669 in 1802 to 23,179 in 1804. Their deaths were augmented more than six-fold. But the different degree in which the famine acted on the women and children of the higher and lower castes is very striking. The deaths of the



females of the higher castes are increased very little more than those of the men; the mortality of children is still less increased; but among the inferior castes, the mortality of women is increased 15 times, and that of children nearly 12 times.

On the native Christians the operation of the famine was only to increase the burials from 184 to 201. This small increase probably affected only the poorest native Christians of Bombay; for there are very few Christians in the neighbouring provinces where the famine raged, and which poured into the island that crowd of fugitives which swelled the Hindu deaths to so tremendous an amount.

One of the most curious results which these documents afford is that relating to the proportions of the two sexes, and to the extent in which polygamy prevails in India. An illustrious philosopher,\* misled by travellers, too much disposed to make general inferences from a few peculiar cases, and pleased to discover a seeming solution of the repugnant systems of domestic life adopted in Europe and in Asia, supposes the polygamy of Eastern nations to be the natural consequence of the superabundance of women produced in warm climates:—Mr. Bruce attempts to support this theory by a statement of a most extraordinary nature. According to him, in Mesopotamia, Armenia, and Syria, the proportion of births is two women (*and a small fraction*) to one man; from Latakia to Sidon it is two and *three-fourths* to one man; from Suez to the Straits of Babelmandel the proportion is fully four to one man, which he believes holds as far as the line, and 30° beyond it.† The confidence with which a private traveller makes a statement so minute respecting such countries is sufficient to deprive it of all authority. Without imputing intentional falsehood to Mr. Bruce (which seems foreign to his character), this statement may be quoted as an instance of that dogmatism, credulity, ostentation, and loose recollection, which have thrown an unmerited suspicion over the general veracity of one of the most enterprising of travellers, as well as amusing of writers. It is singular that reflections of a very obvious sort did not check such statements and speculations. In a country where there were four women to one man, it is evident that nothing less than the practice of polygamy to to the full extent of Mahomet's permission could have provided for the surplus of females; but it ought to have been almost equally evident, that to support more than one wife and family must be beyond the power of the laborious and indigent classes. Though the necessaries of life be fewer, and attainable with less labour in warm than in cold climates, the effects of bad government more than counterbalance the bounty of nature. To suppose that an Egyptian Fellah could support three or four times as many women and children by his industry as a French

\* De l'Esprit des Loix, liv. xvi. chap. 4.

† Travels, ii. 181. 2d Edit.

or English labourer, would be the height of extravagance. Polygamy must in the nature of things be confined to the rich; and must, therefore, depend not on physical causes, but on those tyrannical systems of government which, sanctioned by base superstitions, have doomed one half of the human race to imprisonment and slavery. But facts are more important than any reasonings, however conclusive. By the report of Mr. Ravenshaw, contained in the very instructive Travels of Dr. Francis Buchanan,\* we learn, that in the southern part of the province of Canara the whole number of inhabitants was 396,672, of whom the males were 206,633, the females 190,039. The same excess of males above females is, he tells us, to be found in the Barra Mahl and other parts of the peninsula where accurate enumerations have been made. The return of deaths in the island of Bombay for nearly eight years establishes the same fact with respect to the whole population, and to each of the classes which compose it.

It is well known that the Mahometans are the only class of men in India who practise polygamy to any considerable extent. Out of 20,000 Mahometans in the island of Bombay, only about 100 have two wives, and only five have three; so inconsiderable is the immediate practical result of a system which, in its principles and indirect consequences, produces more evil than perhaps any other human institution, so insignificant is the number of those for whose imagined gratification so immense a body of reasonable beings are degraded and enslaved.

It is remarkable that the only apparent superiority of the number of females is in some of the returns of the Christian congregations, where polygamy is of course unknown. It is reasonable to refer this small exception to accidental causes, which further inquiry will probably discover.

In all the other castes the equality of the sexes apparent in the list of burials is a sufficient proof against the prevalence of polygamy; since it is well known how few natives of India are unmarried.

Polygamy arises from tyranny, not from climate; it degrades all women for the sake of a very few men. And the frame of society has confined its practice within such narrow limits that it never can oppose any serious obstacle to beneficial changes in the moral habits, domestic relations, and religious opinions of the natives of India.

#### No. I.

*Register of Dead Bodies burned and buried in the Island of Bombay, from the Year 1800 to the Year 1808 inclusive.*

*Abstract.*—In the year 1801, 4,835; 1802, 5,297; 1803, 8,320; 1804, 25,834; 1805, 10,347; 1806, 6,440; 1807, 5,834; 1808, 7,517.

\* Fran. Buch. Mysore, iii. 8.

## Register of the Dead Bodies burned and buried in 1800, 1801, 1802.

	Hindoos buried.			Hindoos burned.			Mussulmans buried.			Parsees.			Esraïl* buried.			Christians buried.			Total.
In 1800.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	
Sept. 26 to 30..	11	11	11	12	1	—	6	6	1	—	—	—	—	—	—	—	—	—	59
Oct. ....	100	38	124	83	48	14	50	26	10	—	—	—	—	—	—	—	—	—	493
Nov. ....	75	43	126	68	43	13	46	19	28	10	—	10	—	1	—	5	4	13	504
Dec. ....	105	72	107	77	47	14	33	36	18	10	—	8	—	—	—	6	4	10	547
	291	164	368	240	139	41	135	87	57	20	—	18	—	1	—	11	8	23	1,603
1801.																			
Jan. ....	77	50	117	59	48	29	32	21	45	9	10	10	1	1	1	11	7	13	541
Feb. ....	56	29	96	42	33	33	37	25	29	7	11	15	—	—	—	11	9	11	445
March...	38	39	160	57	40	43	34	20	37	5	5	28	—	—	—	8	4	14	534
April. ..	37	27	188	39	24	43	27	25	38	8	3	33	—	1	5	8	5	10	521
May ....	38	29	157	40	21	24	26	25	43	9	3	36	2	1	2	9	5	7	477
June ....	31	27	120	29	18	21	16	11	32	4	2	19	2	1	4	8	4	4	353
July ....	31	36	68	43	28	25	18	23	23	7	3	13	—	—	1	7	2	4	332
Aug. ....	19	30	74	37	32	21	29	15	35	10	2	10	—	—	2	8	7	6	337
Sept. ....	27	26	63	42	37	19	23	12	13	1	—	—	9	7	12	6	7	5	309
Oct. ....	31	30	58	38	31	9	25	20	21	—	1	1	8	5	9	5	2	4	298
Nov. ....	30	30	47	48	28	16	30	23	16	2	—	—	2	1	13	6	6	4	302
Dec. ....	43	38	69	51	37	19	41	16	27	—	—	1	7	7	8	9	12	1	386
	458	391	1217	525	377	302	338	236	359	62	40	166	31	24	60	96	70	33	4,835
1802.																			
Jan. ....	35	27	77	45	40	18	28	22	33	13	6	16	—	—	1	10	3	4	379
Feb. ....	36	25	76	56	27	26	29	15	21	9	2	14	—	—	—	7	6	7	356
March...	47	34	88	46	29	18	30	18	35	10	9	10	—	1	—	11	8	10	404
April. ..	31	26	89	40	22	23	30	22	27	5	4	15	—	1	1	2	2	6	351
May ....	62	41	93	71	41	36	44	25	35	7	3	7	1	1	1	7	10	3	488
June ....	67	50	196	49	44	24	23	27	37	10	4	11	—	2	2	7	3	—	496
July ....	59	36	140	39	29	33	38	29	52	11	4	14	1	2	4	4	4	4	503
Aug. ....	47	35	95	48	42	22	41	22	40	5	6	18	2	2	1	3	11	7	447
Sept. ....	54	40	99	67	48	35	43	27	31	12	4	16	1	2	1	5	7	2	496
Oct. ....	53	37	102	55	40	35	36	26	29	4	4	12	—	2	—	1	2	4	442
Nov. ....	63	50	119	57	37	21	28	38	32	6	5	15	2	—	1	2	5	2	485
Dec. ....	58	47	120	53	32	19	32	34	20	8	4	8	—	2	1	5	4	6	453
	612	448	1234	626	431	315	402	306	392	100	55	156	7	15	13	64	65	55	5,297

\* Jews.

Register of the Dead Bodies burned and buried in 1803, 1804, 1805.

1803.	Hindoos buried.			Hindoos burned.			Mussulmans buried.			Parsees.			Esraill buried.			Christians buried.			Total.
	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	
Jan.....	64	52	130	68	55	28	39	34	26	10	5	14	—	1	1	5	7	4	543
Feb.....	73	52	20	62	49	134	33	17	29	4	7	8	2	2	2	2	3	5	504
March..	83	68	36	95	76	175	59	35	31	7	5	5	1	—	3	5	2	2	689
April...	99	58	35	79	69	204	47	50	52	6	4	14	2	1	—	6	5	5	736
May....	84	63	52	99	79	241	39	40	41	4	2	19	—	—	7	6	6	12	794
June...	72	64	43	97	97	223	36	36	54	7	7	10	2	—	2	3	6	5	764
July....	82	54	37	86	67	210	47	43	43	7	15	17	—	—	4	5	3	5	725
Aug.....	87	70	47	107	96	221	53	55	52	11	13	18	—	1	1	2	11	7	852
Sept.....	83	67	50	133	106	187	46	43	43	6	7	12	—	3	—	5	4	3	798
Oct.....	76	59	37	117	100	136	46	44	36	7	9	10	—	—	2	6	5	5	695
Nov.....	70	60	40	83	82	121	48	39	34	5	8	18	—	1	2	4	4	6	625
Dec.....	72	52	40	83	76	120	38	31	33	3	10	15	1	3	1	4	8	5	595
	945	719	567	1109	952	2000	531	467	474	77	92	161	8	12	25	53	64	64	8,320
1804.																			
Jan.....	87	53	43	83	82	121	33	29	24	5	9	13	3	1	—	3	5	6	600
Feb.....	152	105	189	76	56	50	44	41	36	12	9	46	—	1	—	4	6	7	836
March..	80	69	64	193	136	247	56	46	46	10	13	39	—	—	—	4	6	9	1016
April...	305	222	426	107	65	59	54	47	52	6	14	28	1	—	4	4	5	6	1408
May....	78	53	38	404	267	564	61	55	47	6	8	15	1	2	3	8	6	6	1628
June....	80	60	38	510	370	575	62	59	57	5	10	28	—	1	—	5	6	8	1874
July....	100	66	57	852	656	807	91	88	84	6	17	24	4	—	2	7	6	7	2877
Aug.....	109	102	88	1228	924	947	102	108	112	12	9	27	1	—	1	9	8	10	3797
Sept.....	402	286	208	1046	839	821	104	100	101	10	14	16	1	—	4	6	8	2	3968
Oct.....	598	492	250	611	566	546	115	106	103	19	6	18	—	—	—	3	4	3	3440
Nov...	358	294	168	404	374	378	107	103	105	28	11	18	4	1	2	4	2	1	2360
Dec.....	323	260	146	343	328	295	99	90	78	20	13	13	2	1	6	6	4	3	2030
	2672	2062	1715	5857	4663	5410	928	872	845	145	133	285	17	7	22	63	66	72	25,834
1805.																			
Jan.....	263	191	81	283	189	206	97	73	69	13	11	16	4	2	8	5	5	4	1520
Feb.....	84	65	37	282	201	193	59	45	54	12	16	13	2	2	2	4	1	3	1075
March..	70	55	26	258	202	196	47	48	50	13	13	25	6	2	—	4	4	4	1023
April...	76	56	45	213	156	195	49	42	36	11	11	32	3	3	2	4	3	3	940
May....	58	44	31	175	149	168	45	37	35	12	15	27	1	2	2	2	1	2	806
June....	41	23	9	186	110	128	44	27	32	14	4	15	—	—	2	—	—	—	635
July....	52	53	23	179	135	156	44	33	26	9	13	15	4	2	2	9	9	7	771
Aug....	55	41	31	190	132	143	54	33	37	8	7	27	1	—	—	4	6	3	772
Sept....	59	47	30	157	135	135	43	46	38	7	4	22	3	2	—	3	6	8	739
Oct.....	62	59	26	151	129	155	50	38	25	6	6	23	—	—	—	4	5	6	745
Nov.....	50	35	19	145	128	117	44	32	35	16	7	13	1	3	—	5	4	2	856
Dec.....	63	54	25	138	110	118	36	33	38	12	13	12	1	1	—	6	2	3	665
	993	723	383	2357	1776	1910	612	487	475	133	120	240	26	19	18	50	46	45	10,347



## Register of the Dead Bodies burned and buried in 1806, 1807, 1808, 1809.

	Hindoos buried.			Hindoos burned.			Mussulmans buried.			Parsees.			Esraill buried.			Christians buried.			Total.
1806.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	Male.	Female.	Child.	
Jan. ....	56	53	25	125	111	135	34	22	25	9	2	9	7	—	2	6	2	1	624
Feb. ....	52	50	24	101	98	96	31	28	25	6	10	11	3	—	—	5	5	2	547
March ..	57	40	19	127	119	133	34	27	16	10	8	13	—	—	—	3	3	3	612
April. .	41	35	20	100	110	136	35	22	25	9	11	13	—	—	—	3	2	5	569
May ....	41	44	24	102	103	156	39	24	29	10	8	9	1	3	1	3	3	6	606
June ....	25	17	5	80	88	126	32	32	27	9	3	11	1	—	4	5	3	3	471
July ....	34	35	19	99	88	147	30	32	32	9	5	9	1	2	—	8	2	7	559
Aug. ....	40	38	26	72	84	133	35	32	30	4	7	9	—	2	—	2	3	2	519
Sept. ....	43	33	23	85	80	159	36	41	46	7	2	11	3	—	—	7	1	3	580
Oct. ....	49	46	13	95	102	197	32	29	33	7	4	14	—	—	—	6	4	10	631
Nov. ....	50	45	20	116	96	170	27	29	28	8	10	13	—	—	—	5	5	2	624
Dec. ....	63	47	23	134	105	153	43	32	30	12	17	17	—	—	4	6	8	4	698
	551	483	241	1236	1184	1741	408	350	346	100	87	132	16	7	11	59	41	48	6,440
1807.																			
Jan. ....	40	30	12	82	74	124	35	24	29	14	8	15	2	1	—	2	4	4	500
Feb. ....	41	35	20	71	74	97	28	20	22	9	12	7	1	1	—	5	4	4	451
March ..	37	19	12	75	60	86	44	27	25	8	9	21	1	—	—	5	4	1	434
April. .	38	30	7	74	67	91	29	17	23	4	11	18	—	1	—	4	4	3	421
May ....	39	29	13	77	78	110	30	24	27	15	5	17	1	2	—	1	3	2	473
June ....	40	27	19	69	65	116	38	25	40	15	9	12	—	—	—	3	5	1	481
July ....	33	38	20	90	76	172	44	35	32	12	10	5	2	3	3	6	7	5	593
Aug. ....	43	46	18	80	74	137	33	34	18	16	15	13	—	1	—	5	2	9	544
Sept. ....	50	33	16	71	69	116	33	28	16	8	8	16	—	1	—	4	5	2	476
Oct. ....	47	38	12	65	70	100	38	33	20	9	11	19	—	2	—	5	2	6	477
Nov. ....	57	39	18	82	59	97	35	26	24	12	10	6	—	1	—	5	5	4	480
Dec. ....	55	36	17	71	65	125	32	26	19	12	10	19	—	—	—	6	8	—	501
	520	400	184	907	831	1371	419	319	295	134	118	168	7	13	3	51	53	41	5,384
1808.																			
Jan. ....	36	34	9	69	61	106	33	27	24	10	4	10	—	1	3	4	5	5	447
Feb. ....	36	32	14	61	44	71	25	27	24	11	6	10	—	—	—	4	2	6	373
March ..	47	40	17	76	70	128	32	30	18	1	8	15	—	—	—	2	4	2	502
April. .	54	44	33	68	58	129	37	24	27	8	19	11	1	—	1	2	2	7	525
May ....	53	31	23	115	63	192	42	41	51	1	7	24	1	—	1	3	1	11	670
June ....	39	41	39	94	60	201	33	37	53	15	10	19	—	1	1	5	9	7	664
July ....	42	41	28	86	71	238	47	41	60	15	11	21	—	1	—	4	3	13	722
Aug. ....	49	38	46	102	87	186	46	40	43	15	11	20	—	—	5	9	8	9	714
Sept. ....	53	58	44	93	102	182	43	41	47	16	13	27	—	1	—	3	4	11	738
Oct. ....	64	43	36	107	108	215	52	54	57	16	11	28	1	1	—	2	2	3	800
Nov. ....	55	40	36	111	97	160	35	27	40	12	6	20	—	2	—	8	6	5	658
Dec. ....	65	47	36	113	86	168	44	34	35	23	13	15	1	1	2	3	6	8	704
	593	489	361	1095	907	1976	469	423	479	171	119	220	4	8	13	49	52	84	7,517
1809.																			
Jan. ....	55	39	33	110	96	160	45	39	35	21	15	19	4	2	1	7	7	7	695
Feb. ....	46	47	27	129	108	160	41	51	55	20	6	5	—	0	1	11	8	13	721



## No. II.

*State of the Mussulman Population of the Island of Bombay, from a Survey by Kazee Shaboodeen Mohuree.*

	Men above 15.	Widows.	Married women.	Male children by wives.	Female children by wives.	Concubines.	Sons of concubines.	Daughters of concubines.	Male slaves.	Female slaves.	Men having two wives.	Men having three wives.	Total.
Kokunee and Mahratta Musaulmans .....	4120	1930	2775	1024	1455	115	30	23	354	551	104 $\frac{2}{2}$	5 $\frac{3}{3}$	13,502
Mussulman water-carriers .....	62	10	25	13	17	—	—	—	—	—	$\frac{1}{2}$	—	129
Butchers who kill goats only (from the Goats)	106	50	70	49	83	—	—	—	9	6	$\frac{1}{2}$	—	331
Koolaba and Mazagao	200	80	100	50	40	—	—	—	15	10	$\frac{1}{2}$	—	499
Mahim .....	247	100	188	129	62	—	—	—	12	20	$\frac{3}{3}$	—	764
	4735	170	3160	2165	1607	115	30	23	390	587	$\frac{11}{2}$	$\frac{5}{5}$	15,225
Men connected with establishments of prostitution .....	200	—	—	—	—	—	—	—	—	—	—	—	200
Prostitutes and females connected with prostitution .....	—	—	—	—	—	—	—	—	—	—	—	—	1200
Memun Mussulmans ..	2000	400	500	400	250	7	—	—	40	50	$\frac{6}{12}$	—	3659
Total .....	6935	2570	3660	2565	1857	122	30	23	430	637	$\frac{12}{2}$	$\frac{5}{5}$	20,284

*Abstract of Totals.*

Men above 15 .....	6,935
Male children by wives .....	2,565
Sons of concubines .....	30
Male slaves. ....	430

Total males .....

9,960

Total females .....

10,324

20,284

Widows .....	2,570
Married women .....	3,660
Female children by wives .....	1,857
Concubines .....	122
Daughters of concubines .....	23
Female slaves .....	637
Wives having one partner .....	240
Wives having two partners .....	15
Prostitutes .....	1,200

Total females. .... 10,324

This was taken in the beginning of 1808. J. MACKINTOSH.

No. III.

*List of Parsee Caste, now Inhabitants of Bombay.*

Men from 20 to 80 years of age. ....	3,644
Women from ditto to ditto .....	3,333
Boys from 20 down to infant child. ..	1,799
Girls ditto ditto .....	1,266

10,042

Bombay, Feb. 28, 1811.

No. IV.

*Account of the Numbers, Births, and Deaths, of the Christian Inhabitants appertaining to the Parish Church of St. Michel, at Mahim, in the Island of Bombay, from Jan. 1800 to Jan. 1810.*

1800. Jan.—Souls. ....	1863	1806. Jan.—Souls. ....	1863
Births ....	62	Births ....	68
Deaths . . .	92	Deaths . . .	73
1801. Jan.—Souls. ....	1863	1807. Jan.—Souls. ....	1892
Births ....	56	Births ....	62
Deaths . . .	66	Deaths . . .	100
1802. Jan.—Souls. ....	1900	1808. Jan.—Souls. ....	1878
Births ....	60	Births ....	67
Deaths . . .	102	Deaths . . .	77
1803. Jan.—Souls. ....	1812	1809. Jan.—Souls. ....	1724
Births ....	83	Births ....	73
Deaths . . .	76	Deaths . . .	112
1804. Jan.—Souls. ....	1877	1810. Jan.—Souls. ....	1712
Births ....	62	Births ....	70
Deaths . . .	127	Deaths . . .	87
1805. Jan.—Souls. ....	1848		
Births ....	55		
Deaths . . .	7		

Parish Church of St. Michel,  
Feb. 25, 1811, at Mahim.

DOM MATHIAS DE MONTE TARIA,  
Vicar.

## No. V.

*List of the Christian Inhabitants of the Church of our Lady of Salvation; of the Births, Deaths, and Living, from Jan. 1800, to Dec. 31, 1810.*

	Births.	Deaths.	Living.
1800 .....	111 .....	89 .....	1580
1801 .....	102 .....	64 .....	1527
1802 .....	77 .....	92 .....	1491
1803 .....	74 .....	90 .....	1504
1804 .....	75 .....	146 .....	1500
1805 .....	67 .....	87 .....	1400
1806 .....	61 .....	75 .....	1300
1807 .....	73 .....	92 .....	1400
1808 .....	106 .....	88 .....	1527
1809 .....	69 .....	78 .....	1520
1810 .....	73 .....	77 .....	1430

*Church of Our Lady of Salvation  
of Mahim, Feb. 23, 1811.*

JOAM DSZA ESILVA.

## No. VI.

*Abstract of the Births, Deaths, and Living of the Church of Our Lady of the Rosary of Mazagon.*

Years.	Births.	Deaths.		
		Men.	Women.	Children.
1800	26	9	11	24
1801	30	5	16	9
1802	29	7	22	18
1803	31	9	13	20
1804	24	8	13	19
1805	17	10	7	10
1806	30	6	9	4
1807	32	15	16	10
1808	26	7	13	14
1809	32	9	14	11
1810	25	8	22	21
1811	2	1	1	1
	304	94	157	161
		Living.		
		252	325	179

	Totals.
Living .....	756
Births .....	304
Deaths .....	412

*Church of Our Lady of the Rosary of Mazagon,  
Feb. 22, 1811.*

## No. VII.

*Account of the Christians of the Church of Our Lady of Hope at Bombay.*

Men .....	1473
Women .....	2280
	<hr/>
	3752

	Deaths.
1801.....	187
1802.....	185
1803.....	218
1804.....	233
1805.....	151
1806.....	138
1807.....	147
1808.....	132
1809.....	168
1810.....	158
	<hr/>

1717 or annually 156

Births 1801 to 1811, 985, or 88 annually.

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ARTICLE VI.*A Memoir on some new Combinations of Prussic Acid.*

By his Excellency the Count Le Maistre, of St. Petersburg.

FERRUGINOUS prussic acid, when assisted by heat, combines with different substances, which, while cold, do not exhibit any affinity for it; and all these combinations are blue, like prussiate of iron. Copper itself, whose dark carmine red prussiate is well known, is also capable of forming a blue prussiate according to the state of oxygenation in which it is, and by means of processes which will be explained in the course of this memoir. The following are these different combinations in the order in which they presented themselves to me during my experiments.

I. *Prussiate of Starch.*

While engaged in making a small quantity of iodide of starch in a porcelain cup, I employed in the preparation water which contained prussiate of potash and muriatic acid in solution. The mixture which I exposed to the heat of a lamp remained colourless till the heat became sufficiently strong to dissolve the starch. It then suddenly assumed a very perceptible green colour. As I was aware of the substances contained in the mixture, I imme-

diately repeated the experiment in such a way as to remove all suspicion of the presence of iron. I took 10 gr. of fine starch which I mixed with an ounce of the water of the Neva. I added 20 gr. of prussiate of potash, and boiled the whole in a matrass. The liquid became transparent, and remained colourless during the whole time of boiling. I then added muriatic acid, which made it immediately assume a green colour as if I had poured into it a solution of iron. The precipitate in 48 hours became of a fine deep blue. I repeated the experiment with different starchy substances, as arrow root, sago, potato starch, and always obtained the same result. Thus starchy bodies, when dissolved in water at the temperature of  $212^{\circ}$ , have the property of forming blue prussiates like iron.

## II. *Prussiate of Gum Arabic.*

I made the same trials with some analogous substances. Ten grains of gum arabic and 20 gr. of prussiate of potash were dissolved and heated together in a matrass with an ounce of water. The mixture remained transparent and colourless during the boiling. Boiling muriatic acid made it pass to a deep green. The precipitate was abundant, and fell down rapidly.

The prussiate of gum is less black than pure prussiate of iron. It is insoluble in water, and in muriatic acid. Concentrated sulphuric acid dissolves it, and destroys the colour; but it appears again when the solution is diluted with water. The prussiate in that case precipitates with all its lustre, and when it is washed and dried, we cannot from its colour distinguish it from common prussian blue.

## III. *Prussiate of Sugar.*

The same process and the same quantities being employed with sugar yielded likewise a blue prussiate, which was at first green, and which, when exposed to the air, assumed a lighter shade than the preceding blues.

To avoid useless repetitions, I shall merely say, that having experimented in the same way with solutions of gelatin, of cochineal, of tannin, of soap, and even of white wax melted and mixed with the prussiate of potash, on decomposing the prussiate by muriatic acid while boiling-hot, I always obtained blue prussiates; while none of these compounds could be formed at the temperature of the atmosphere.\*

Of all these substances cochineal is the one which resists most the action of prussic acid, the combination not taking place till after half an hour's boiling. This colouring matter, which the mineral acids and alkalies do not decompose, becomes green by its union with prussic acid. The precipitate resembles the juice

\* It may be thought that these substances are coloured by the iron of the prussic acid as by a tincture; but the gum and starch become insoluble in water, which indicates a true combination, and the cochineal becomes green, and not violet.



of herbs, and soon passes into a perfect blue. The residual liquid, when there is an excess of cochineal, preserves its red colour; the prussic acid taking only the quantity necessary for its saturation.

#### IV. *Prussiate of Charcoal.*

As all the substances of which I have spoken contain a great quantity of charcoal, it was natural to try the same thing with this last substance. To obtain a charcoal in a state of minute division, I charred white paper free from size by putting it into concentrated sulphuric acid, and exposing the acid to heat. The black matter thus formed was diluted with a great deal of water, and decanted without filtration into a matrass, which I placed over the flame of a spirit of wine lamp. I then poured into it while boiling a portion of prussiate of potash, and the mixture became dark green. The prussiate did not assume a blue colour till after eight days exposure to the air. That the paper may be completely charred by the sulphuric acid, it must be slightly moistened before being plunged into it.

This, however, was not a pure prussiate of charcoal; for as the sulphuric acid chars the paper without effervescence and without smell, this species of solution contains all the principles of the ligneous substance.

To obtain a pure prussiate of charcoal, I took 30 gr. of the charcoal of the birch which had remained for a fortnight in an alkaline ley. I pounded it while still moist upon an unpolished piece of glass, with concentrated sulphuric acid. I put altogether three ounces of water, and boiled the mixture in a matrass. I then added to the liquid 60 gr. of prussiate of potash, continued the boiling for about an hour, adding water in proportion as it evaporated. A dark-green prussiate was formed, which became blue after five or six days exposure to the air.

If the dried prussiate is not of as fine a blue as the best prussian blue, it must be repeatedly moistened and dried, which will produce the colour desired.

When the process was repeated with muriatic acid instead of sulphuric, the result was the same; but when newly formed charcoal was employed, no combination took place.

The formation of this prussiate was accompanied with frequent anomalies, depending doubtless on the different states in which the charcoal is at the time in which it is employed, and upon minute circumstances which I probably overlooked. If instead of boiling the mixture for an hour, as I have described, it be poured after two minutes boiling into a bottle furnished with a ground-stopper, and placed well corked in a stove heated to  $122^{\circ}$  or  $144^{\circ}$ , it will be found completely formed after an interval of eight or ten hours. I put my bottles in the evening into a Russian stove, and next morning found a green prussiate formed.

It cannot be doubted that if the charcoal could be presented to the prussic acid in a state of perfect solution, the combination would take place instantly, as happens with the gums.

When the prussiate of charcoal is dried, it has a fine brilliant blue colour, like prussian blue containing alumina, which appears singular, if we consider the black colour of charcoal. It dissolves in cold sulphuric acid of commerce without losing its colour. The solution appears green when the acid begins to act, as is the case with indigo; and when it is diluted with water, the prussiate precipitates.

If we employ very concentrated sulphuric acid, the colour disappears on solution, and the liquid becomes yellowish; but when water is added, the prussiate precipitates of its original colour. It is insoluble in water and in muriatic acid, which merely divides it.

These properties of prussiate of charcoal belong likewise to those of starch, gum arabic, and cochineal, which are probably of the same nature.

The analogy which exists between this prussiate and indigo is remarkable. They have both the same colour. The absorption of oxygen makes them both pass from green to blue. They are both soluble in sulphuric acid, and finally they are composed of the same principles; namely, of a great proportion of carbon combined with azote, hydrogen, oxygen, and iron; but in proportions, or, perhaps, only in an order of composition which gives them different properties.

After having formed blue prussiates with the oxide of carbon and several of its compounds, I thought of subjecting to the same process the earths which had not yet been united to this acid, and the simple substances which I could procure.

#### V. *Prussiate of Sulphur.*

As sulphur unites easily with potash, I formed a sulphuret with 10 gr. of flowers of sulphur and 20 gr. of prussiate of potash in a small matrass, which I heated by a spirit of wine lamp. When I thought that the sulphuret was formed, I broke the matrass in a vessel of boiling water mixed with muriatic acid. The sulphuret, which had acquired no colour, was decomposed in the water, and the prussiate of sulphur made its appearance in fine green flocks in the acid liquid. A few days are sufficient to give it a fine deep blue colour.

This prussiate has a green colour only when the sulphuret is made in close vessels. If it be made in an open crucible, and if it be allowed to burn for some time stirring it with a glass rod, the prussiate is blue the instant it is developed in the muriatic acid; so that the portion of the sulphur which burns is sufficient to bring the rest to the point necessary for forming prussian blue. I shall prove afterwards that no prussiate becomes blue unless oxygen and iron enter as constituent parts of prussic acid.

The following process likewise furnishes a prussiate of sulphur of a very beautiful colour.

I boiled in water one part of hydrosulphuret of potash and one part of prussiate of potash. I added weak muriatic acid at a boiling temperature, which immediately produced the green combination. No sensible portion of sulphuretted hydrogen was developed.

#### VI. *Prussiate of Phosphorus.*

Phosphorus has a great deal of affinity for prussic acid. To produce a combination it is sufficient to boil some grains in a solution of prussiate of potash, and to pour muriatic acid into the liquid. It becomes muddy, and assumes a green colour. The boiling is to be continued till no more melted phosphorus can be seen at the bottom of the matrass. The prussiate has then the colour of Scheele's green, and in process of time becomes blue.

It would seem that phosphorus can unite in several proportions with prussic acid. If we employ a weak solution of prussiate of potash, the colour is less intense, and passes with difficulty to blue. If we employ a concentrated solution, and pure muriatic acid, the prussiate, when dried, is absolutely black, and becomes luminous. The excess of phosphorus burns slowly, and leaves a dark-violet coloured matter, which gradually changes into a perfect blue.

Prussiate of phosphorus, when well made, is more beautiful than prussiate of iron, and is neither purple nor greenish.

#### VII. *Prussiate of Gold.*

A solution of ducat gold diluted with much water, treated during its boiling with prussiate of potash, gave immediately a copious blue precipitate, which was deposited rapidly, and had a very fine colour.

#### VIII. *Prussiate of Silver.*

As all the nitric acid solutions have a tendency to give a green shade to the prussiates derived from them, we obtain only an imperfect blue with nitrate of silver; but we get a very fine blue by operating on the muriate of silver newly precipitated by muriatic acid. It is washed upon the filter, and boiled in water, before it is coloured by light. Muriatic acid is added, and then prussiate of potash. A prussiate is formed of a clear and brilliant blue, which becomes gradually more intense. This is the prussiate of muriate of silver.

#### IX. *Prussiate of Tin.*

The disoxygenizing property of muriate of tin renders the formation of blue prussiates with it difficult; but we obtain them with facility when we operate upon the oxide of tin precipitated from the nitrate by an alkali, well washed, and employed while

still moist. The process is the same as in the preceding experiment, excepting that the prussiate of potash must be added to the mixture before introducing the muriatic acid.

### X. *Blue Prussiate of Copper.*

Copper presents the singular phenomenon of being able to furnish two permanent prussiates of different colours.

If we pour a strong solution of prussiate of potash on pure copper filings, and then add a sufficient quantity of muriatic acid to decompose the prussiate, and to dissolve a portion of the copper, the metal dissolved in presence of the prussic acid forms a very beautiful blue prussiate. This combination does not, like the preceding, depend upon the temperature. Heat merely increases the rapidity of its formation.

When we treat granulated tin in the same manner, we obtain likewise a blue prussiate, but we must employ nitromuriatic acid and heat.

### XI. *Blue Prussiate of Mercury.*

To form the blue prussiate of mercury, the red oxide of that metal is boiled in water, and a solution of prussiate of potash previously decomposed by muriatic acid is added to it. The iron contained in the prussic acid changes its condition so as to become proper for forming the blue insoluble prussiate of mercury; while the acid without iron forms a colourless soluble prussiate. This proves sufficiently that iron is a constituent part of the prussic acid employed in these experiments. The blue prussiate of mercury, when first formed, is light green, and passes slowly to blue by exposure to the air.

### XII. *Prussiate of Iodine.*

If we boil iodine in a solution of prussiate of potash, adding muriatic acid, we obtain a blue prussiate with a shade of purple of the greatest beauty. When first formed, it is light green, and requires a long exposure to the air to give it a blue colour.

### XIII. *Prussiate of Alumina.*

The opinion that alumina forms no combination with prussic acid is so generally established that when I began my trials with this earth, I had little expectation of success. In fact the sulphate of alumina precipitated by prussiate of potash gives no colour either when cold or hot. However, I observed that when the salts in the mixture were concentrated, the alumina, after long boiling, assumed a very distinct blue colour. The following trial furnished me with a more complete combination.

I pulverized together equal parts of sulphate of alumina and prussiate of potash. I introduced them into a small matrass without water, and heated them over a spirit of wine lamp to cause them to melt in their water of crystallization. The mixture became blue as soon as it began to swell. I regulated the



heat in order not to decompose the prussiate of alumina, and when the tint appeared to me equal through the whole mass, I diluted it with boiling water. The prussiate, which was of a light and dirty blue, immediately assumes a fine colour, and forms a copious precipitate similar to prussiate of iron; but which seems somewhat soluble in water, to which it gives a greenish tint.

As alumina has been long employed by the makers of prussian blue along with prussiate of potash without any such combination being observed, I repeated the same experiment several times to be certain of its accuracy, and I likewise made another experiment which appeared to me conclusive.

I precipitated the alumina from the sulphate by means of carbonate of potash, I washed the earth on a filter, and dissolved it, while yet in a gelatinous state, in muriatic acid. I heated the solution which was very acid till it boiled, and then threw into it boiling hot prussiate of potash. The effect was the same as if it had been poured into a solution of protosulphate of iron. Instantly a very fine green colour was developed, which in two days was changed into an intense and perfect blue. We have seen that sulphate of alumina exhibits different phenomena, which is doubtless the reason why this compound has never been observed in the prussian blue manufactories.

Alumina then has the property of forming a blue combination with prussic acid, but only at a temperature not lower than  $212^{\circ}$ .

#### XIV. *Prussiate of Silica.*

To form this prussiate, I pounded in a glass mortar 50 gr. of white glass from a barometer tube with 150 gr. of calcined potash. I put the mixture into a covered crucible, and kept it red hot for an hour. A frit was formed, which was almost completely soluble in muriatic acid. I filtered the transparent and colourless solution, and raised it to a boiling temperature. I then added 40 gr. of prussiate of potash, which gave a deep-green colour to the liquid. After eight days exposure to the air, it became a blue prussiate.

The same process performed with pulverized quartz, prepared for porcelain, gave the same result. The prussiate of silica is blacker and less beautiful than prussiate of alumina.

#### XV. *Prussiate of Carbonate of Lime.*

Some salts, insoluble, or scarcely soluble in water, form likewise blue combinations with prussic acid without undergoing decomposition.

I boiled water containing carbonate of lime diffused through it, and mixed with it a liquid containing prussiate of potash with an excess of muriatic acid. The prussic acid immediately united with the carbonate, and the prussiate formed became bluish-black after some days exposure to the air.

It was insoluble, in cold muriatic acid. Concentrated sul-



phuric acid disengaged a great deal of carbonic acid, and formed a sulphate without destroying the blue colour.

#### XVI. *Prussiate of Sulphate of Lime.*

Sulphate of lime appears to me one of the substances which has the greatest affinity for prussic acid of those which I tried. I precipitated muriate of lime by sulphuric acid diluted with seven or eight times its weight of water. I heated it to the boiling temperature, and added prussiate of potash. There was immediately formed a prussiate of a fine green, which passed very speedily to blue.

Calcined sulphate, or fine plaster of Paris, may be employed to form this prussiate. It is diffused through a great quantity of water, and then a solution of prussiate of potash having an excess of muriatic acid previously added to it, is poured in. Boiling causes it to assume a green colour.

When the prussiate of sulphate of lime has a strong blue colour, and contains a sufficient quantity of prussic acid, it is insoluble in water, and in the mineral acids: 15 gr. of prussiate of potash were sufficient to give to 100 gr. of calcined plaster a very distinct blue colour.

The sulphates of strontian and barytes newly precipitated and treated like that of lime, likewise furnished deep blue prussiates.

#### XVII. *Prussiate made with common White Clay.*

The dry earths and the metallic oxides in powder, when they are sufficiently divided, combine very well with prussic acid, when assisted by heat.

I mixed with water, fine white clay employed at St. Petersburg for the manufacture of porcelain, and which does not contain an atom of iron. I decanted off the finest parts, which I boiled with prussiate of potash without producing any change in the colour. The addition of muriatic acid made the mixture become green. After a quarter of an hour's boiling, I poured the prussiate formed into a plate. It had a fine green colour, and became blue after some days exposure to the air.

This prussiate may become useful in the arts. It has the advantage of being always of a distinct blue colour whatever be the proportion of acid employed to form it. This is not the case with iron, which gives yellow and green subprussiates.

#### XVIII. *Prussiate formed with Greenish Grey Clay.*

This clay, which is found abundantly in the neighbourhood of St. Petersburg, and which I conceive to be coloured with a little chlorite, when treated like the white clay, gave also a beautiful blue prussiate. As it contains iron, its prussiate is nearly the same as common prussian blue. It requires longer boiling than the preceding, because it is difficult to destroy the green

colour of the earth ; but when it is formed, it is perfectly similar to the finest prussian blue of commerce.

### *Observations.*

When we review the facts contained in this memoir, we may observe, that when the combinations of prussic acid with carbonate of lime, sulphates of lime, barytes, and strontian, pass from green to blue by the slow absorption of oxygen from the atmosphere, it is not, in all probability, the bases of these prussiates which are oxidized ; for the carbonate and the sulphate of the earths are not capable of uniting with an additional dose of oxygen. It is probable then that it is the prussic acid itself which unites with the oxygen in this case.

We find a proof of the oxidation of this acid in its combination with indigo. The boiling sulphate of indigo precipitated by prussiate of potash is all at once disoxygenized, and assumes a green colour, as when it is treated by sulphate of iron in the dyer's vat.

It is likewise very probable that in all the blue metallic prussiates the prussic acid is oxidated ; while the bases are at a minimum of oxidation, and this is evident for the blue prussiate of copper. We see in fact that the protoxide and peroxide of copper in all their solutions form red prussiates ; while in the blue prussiate (experiment 10), the copper dissolved in contact with the prussic acid is seized upon in a nascent state at a degree of oxidation which is doubtless less than that of all the known oxides of copper, which give red prussiates.

If the peroxide of iron gives a blue prussiate at the instant of its formation, the reason is that it contains enough of oxygen to saturate the acid while it passes into the state of protoxide. while the peroxide of mercury which contains only 0.10 or 0.15 of oxygen forms a green prussiate. Now as it is known that mercury is incapable of combining with a greater quantity of oxygen, it follows that when the green prussiate passes into blue, it is not the base which absorbs oxygen, but the prussic acid itself.

Sulphur, phosphorus, and carbon, form green prussiates, because they cannot furnish the requisite quantity of oxygen to the prussic acid. It is obvious that these radicals cannot pass to a maximum of oxidation, when their prussiates change from green to blue, because, when saturated with oxygen, they become acids. This is the case also with those substances which contain much oxygen in such a degree of union that the prussic acid is unable to abstract it. These substances combine green, as the earthy sulphates, and the oxides of aluminum and silicon.

By means of this theory, which explains all the facts, the phenomena of the two prussiates of mercury, one soluble and colourless, the other blue and insoluble, are easily understood.

When in order to make the soluble prussiate of mercury, we boil prussian blue with red precipitate, a change of bases takes place by double affinity. The mercury has a very great affinity for prussic acid; and the iron of prussian blue, which is at a minimum of oxidation, has likewise a great affinity for oxygen. Hence it happens that the iron deprives the acid of oxygen, and passes into the peroxide; while the mercury seizes the pure prussic acid without oxygen and without iron. In this last state the prussic acid drawn from mercury has no *cyanic* power, and remains mixed with sulphate of iron without forming a prussiate.

I shall terminate this memoir by an experiment, the result of which gives great probability to this conjecture.

If the prussic acid, when it combines with the peroxides, has really the property of absorbing a portion of their oxygen, and of bringing them to the state of protoxide, it ought to act in the same way on the oxide of the red prussiate of copper, and make it pass to blue, on the supposition that in the blue prussiate the oxide of copper is at a minimum of oxidation.

To determine this point, I mixed beautiful red prussiate of copper (newly precipitated and washed on the filter) with water. I added a little muriatic acid, and boiled the liquid. The colour appeared to become more red. I then threw in some crystals of prussiate of potash, and had the pleasure of seeing the mixture speedily pass to the finest blue.

This experiment seems to me to furnish a double proof, both that prussic acid does not produce the blue, except when it is oxidized, and that the bases of the metallic prussiates are at the minimum of oxidation. We may, therefore, distinguish three different well marked states in prussic acid. 1. The pure acid without oxygen and without iron, such as it exists in the soluble prussiate of mercury. 2. The ferruginous acid without oxygen, such as it exists in the prussiate of potash and in the green prussiates. 3. The ferruginous and oxygenized acid, such as it exists in all the blue prussiates.

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## ARTICLE VII.

*Observations on Gehlenite, made during a Series of analytical Experiments upon this Mineral; which prove that it contains Potass.* By Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, &c. In a Letter to the Editor.

(To Dr. Thomson.)

SIR,

Cambridge, Oct. 4, 1819.

AMONG the more remarkable properties of minerals is that of their *gelatinization* in acids. This property, which is confined

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to a few bodies, I have always supposed to be owing to the presence either of an *alkali*, or of an *alkaline* earth, in stones containing *silica*. Very often an attention to this property alone is sufficient to make known the presence of *potass* or of *soda* in a mineral. If there be exceptions, it is only where *zinc* or *lime* is present with *silica*; but in the instances of *needle-stone* and *datolite*, which both exhibit the most perfect and transparent jelly upon the action of acids, and both contain *lime*, I have detected also the presence of *soda*. In a case of minerals which I lately received from the *Tirol* there were some specimens of the substance called *Gehlenite*, whose properties and constituents have not, I think, been either adequately or accurately stated in the accounts published of this mineral. In the fifth edition of your *Chemistry* you have placed it at the head of the *feldspar* family; and that this is its proper situation will, perhaps, appear evident from the discovery I have now made of an *alkali* in the stone; when added to the nature of its other constituents, and also to its crystallization, which, in the examples I possess, does not exhibit rectangular, but oblique-angled parallelopipeds.

The purest crystals which I have been able to detach from my specimens of *Gehlenite* were imbedded in a matrix containing *carbonate of lime*. The same *limestone* is also imbedded as extraneous matter in the crystals, together with *oxide of iron*. And to the presence of these *foreign* bodies, as in the example afforded by *Fontainbleau carbonate of lime* (according to a well-known theory of the celebrated *Bournon*), may be owing the simplicity observable in the form of the crystals themselves; which is nearly that of the primary form of *feldspar*; but improperly called *cubes*, in the accounts published of this mineral; owing, perhaps, to the minuteness of the crystals, which prevented a more accurate observation. Observing that these crystals, when pulverized and exposed to the action of *muriatic acid*, were converted into a perfectly transparent jelly, I suspected that an *alkali* might be present, which proved to be true. For the manner of detecting it, I am indebted entirely to the instruction which I received from *Dr. Wollaston*, whose valuable experiment, applicable at all times to stones acted on by acids, I will endeavour to describe, previously to giving any further observations of my own. The gelatinous substance, contained in a watch-glass, was exposed to a temperature not exceeding that of boiling water, until it became perfectly dry. In this state, if there be present *potass* or *soda*, small cubic crystals may sometimes be discerned with a lens; and such crystals were visible in the present instance. Distilled water being then added, the *silica* was separated, and collected upon a filter. The solution which passed the filter, and which contained the *muriates*, was then evaporated to dryness; and the dry mass being exposed to a high temperature, the *muriates* of *alumina* and *iron* were decomposed; and distilled water being again added, their bases



were separated, and collected upon a second filter. The next process related to the separation of the *lime*, which *Gehlenite* is known to contain. To effect this, *carbonate of ammonia* was added to the solution which had passed the filter; and *carbonate of lime*, being precipitated, was collected upon a third filter. The solution now contained the *muriates of ammonia* and of the *alkali* before mentioned. Being evaporated to dryness, a sufficient degree of heat was communicated for the sublimation of the *muriate of ammonia*, which came away in white fumes. Upon adding a few drops of distilled water to the residue, and using *muriate of platinum* as a test, a precipitate was caused by the solution, and the presence of *potass* thereby fully demonstrated. Upon adding also *nitric acid* to the *alkaline* solution, crystals of *nitrate of potass* became visible. After this satisfactory experiment, the remaining observations will be almost superfluous. Indeed had I been earlier in possession of the information I received from *Dr. Wollaston*, the method I pursued in the analysis of *gehlenite* might have been rendered much simpler and shorter. I will, however, state it with as much brevity as I can from the notes which I then made.

#### *Gehlenite.*

**External Characters.**—Crystallized in oblique-angled parallelopipeds, nearly approaching to the cubic form. Crystals containing imbedded particles of *carbonate of lime*, appearing in white specks; exhibiting also *ochreous* surfaces as if in a state of incipient decomposition; set in all directions; generally small; major diameter of the largest  $\frac{1}{10}$ ths of an inch; minor diameter,  $\frac{2}{10}$ ths; showing a tendency to cleavage on the solid angles. Colour, greenish-grey; lustre, resembling that of *pitchstone*. Brittle, but hard when reduced to small fragments. Specific gravity, in distilled water (temperature  $59^{\circ}$  of Fahr.), 2.71.

**Chemical Characters.**—Colour, by trituration, dingy-white. Before the blow-pipe fuses into a gumboge-yellow glass, which, by continuance of the heat, becomes a dark cinder. Effervescence in acids and gelatinization. *Lime* copiously precipitated from the solution by *oxalate of ammonia*, and *iron* by tincture of galls.

**Analysis.**—One hundred grains, triturated in a porphyry mortar, were placed in a covered *platinum* crucible, and exposed to a smart red heat during half an hour. When taken out of the crucible, a loss of weight was observed equal to six grains, owing to the expulsion of *water of absorption*.

A. To the remaining 94 gr. were added about four times their weight of *carbonate of soda*, and the whole being placed in a silver crucible was surrounded with sand in a second earthenware crucible, and exposed to a red heat during an entire hour. Being then removed from the furnace, and allowed to cool, the cover was taken off, and distilled water added by degrees to soften



the mass, which, little by little, was poured into *muriatic acid*. As soon as all effervescence had ceased, more distilled water was given to it; and the whole being placed in an evaporating dish, the excess of acid was volatilized, and water again added; the whole being then thrown upon a filter, the substance which remained upon the filter was washed (until the washings effected no change of colour in *litmus paper*), and afterwards calcined. It proved to be pure *silica*, and weighed 29.50 gr. making no allowance for loss.

B. The *silica* having been separated, as mentioned in A, an excess of the *hydrate of ammonia* was poured into the solution, whereby a copious precipitate was caused; this, being collected on a filter, was carefully washed until the washings did not alter the colour of *litmus paper*. It consisted of *alumina* and *oxide of iron*.

C. The *alumina* and *oxide of iron*, precipitated from their *muricates* by the process mentioned in B, were taken off the filter in a gelatinous state and heated, according to the method recommended by *Thenard*,\* in a capsule, with a great excess of caustic liquid potass, which dissolved the *alumina*, but had no action upon the *oxide of iron*. The capsule being then removed from the fire, and the liquid cooled down to 100° of Fahr. and filtered (the filter being washed until it ceased to exhibit signs of *alkalescence*), the *oxide of iron* remained on the filter, and was removed with an ivory knife, and dried, and calcined. It afterwards weighed 12.20 gr. making no allowance for loss.

D. The washings from B being added to the liquid whence the *alumina* and *oxide of iron* were precipitated, and which contained all the *lime* in the state of a *muriate*, subcarbonate of potass was added to it to effect the decomposition of this *muriate*. A copious precipitate of *subcarbonate of lime*, containing a trace of *magnesia*, ensued; and this being washed, dried, and exposed to a white heat in a platinum crucible during half an hour, afterwards weighed 27.80 gr. making no allowance for loss.

E. The next operation related to the obtaining the *alumina*, which had been dissolved in the alkaline solution mentioned in C. This liquid was first saturated with *muriatic acid*; then an excess of the *hydrate of ammonia* being added, the *alumina* was all precipitated. After being carefully washed, dried, and calcined, it weighed 14.50 gr. making no allowance for loss.

In all these experiments, no allowance whatever has been made for any loss which, in a slight degree, must be sustained where filters are used; and filters here were unavoidable, owing to the partial gelatinization of the precipitates, especially in that of *alumina*. I prefer, however, stating the results exactly as I obtained them by weight, leaving the deficiency to go to the

\* *Traité de Chimie*, tom. iv. p. 122. Paris, 1816.

account of the *alkali*, and by no means adding to the amount of the constituents by any conjectural addition.

There remains now only two more substances to be added to the list; namely, *magnesia* and *potass*.

F. The first, i. e. the *magnesia*, was separated from the *lime* by a process recommended by Dr. *Henry*, of *Manchester*;\* namely, by adding to the residue collected in D more than its own weight of strong *sulphuric acid*; afterwards applying a sand heat until the acid ceased to rise; and then raising the heat so as to expel the excess of the acid. The dry mass was then digested in twice its weight of cold distilled water, to which I added a few drops of *alcohol*. This dissolved the *sulphate of magnesia*, and left the *sulphate of lime*, which was collected on a filter. The *magnesia* was then precipitated from the *sulphate* by the *carbonate of potass* in a heat approaching to 212° of Fahr.; but the whole of the precipitate of the *carbonate of magnesia*, after being washed and dried, weighed only a single grain, allowing for base only 0·25.

From all the preceding experiments, the constituents of *gehlenite* may be thus given:

	Grains.
Water of absorption . . . . .	6·00
Silica, see A . . . . .	29·50
Oxide of iron, see C. . . . .	12·20
Lime, deducting magnesia, D. . . . .	27·55
Alumina, E . . . . .	14·50
Magnesia, F . . . . .	0·25
Potass, and loss . . . . .	10·00
	<hr/>
	100·00

I remain, &c. &c.

EDWARD DANIEL CLARKE.

## ARTICLE VIII.

*Observations on a Memoir by the Abbé Haüy on the Measurements of the Angles of Crystals, and on another by M. Cordier on the Blue Carbonate of Copper.* By H. J. Brooke, Esq. F.R.S. &c.

London, Nov. 8, 1819.

IN the *Annals of Philosophy* for June last, a translation is inserted of a memoir by the Abbé Haüy, on the measurements of the angles of crystals, in reply to some observations by Mr. W. Phillips, published in the *Geolog. Trans.* on the importance

\* *Elements of Chemistry*, vol. ii. p. 488. Lond. 1815.

of ascertaining with precision the angles of the primary forms of crystals, in order to determine their secondary forms with the requisite degree of accuracy. Some remarks in Mr. Phillips's paper, on the comparative value of the reflective and common goniometers in affording accurate measurements of the angles of crystals, appear to have excited a degree of anxiety in the mind of the Abbé lest his theory, which had availed itself only of the common goniometer, should suffer from any disrepute attaching to that instrument. And in the memoir alluded to, he endeavours to show, that in connection with his theory, the instrument he uses is sufficiently precise in its results, although it does not determine the value of an angle within one-half or one-third of a degree, and is hence greatly inferior in accuracy to the reflective goniometer.

It is true that the application of his original and ingenious theory to the measurements afforded by the comparatively imperfect instrument he has used, has, in many instances, led to a nearly accurate determination of the angles of the crystals he has examined: but it contains, nevertheless, a principle which in other instances has conduced to error, and which may affect its worth as a theory more than any consideration of the comparative merit of his goniometer.

This principle is an imaginary simplicity which he supposes to exist naturally in the ratios of certain lines either upon or traversing a crystal, and which in his view assumes the character of a limit to its natural dimensions; and he is disposed to regard generally the disagreement of an observed measurement with this character rather as an error of the observation than a correction of his theoretic determination.

The well-known error to which an adherence to his principle of ideal simplicity has led this philosopher in his determination of the primary form of carbonate of lime amounts to 37 minutes of a degree; and as he has assigned to the magnesian and ferri-ferous carbonates the same angle as to the simple carbonate, the error with regard to these is still greater, as will appear from a statement of the respective measurements of each, of which the two lower ones were first given by Dr. Wollaston.

	Observed angle by reflect. goniometer.	Theoretic angle.	Error.
Carbonate of lime . . . .	105° 05' .. *	104° 28' . . . .	0° 37'
Magnesian carbonate. . .	106 15 . . . .	104 28 . . . .	1 47
Ferri-ferous carbonate. . .	107 00 . . . .	104 28 . . . .	2 32

Indeed his application of this principle of simplicity seems limited rather by the quantity of error that may be admitted with tolerable safety into the calculation than by that attempt at absolute precision which appeared to Mr. Phillips so essential to the accurate determination of secondary forms.

\* 104° 28' 40' is the precise angle given in the *Tableau Comparatif*.

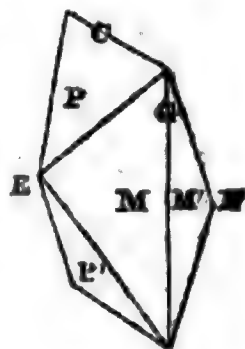
The example I have selected of carbonate of lime evinces that this theoretic simplicity does not always exist in nature; and when we observe that it leads to an error amounting to more than double the difference between two known species of minerals, the magnesian and ferriferous carbonate of lime; this difference being only  $15'$ , and not knowing that there are not other distinct species approaching still nearer in their measurements, a theory which is satisfied with an error of  $37'$  does not appear to have attained to that degree of perfection which warrants its being regarded as "*sufficiently near the truth.*"

But the intelligence which has conceived this theory can easily remedy its imperfections; and we may hope that the second edition of his Mineralogy, which the scientific author is preparing for the press, will rather adopt and reason upon the dimensions which nature has given to crystals than clothe them with an imaginary character which must be erroneous in proportion as it deviates from nature.

The immediate occasion of these remarks is the appearance of a memoir by M. Cordier on the blue carbonate of copper, in the first part of the Annales des Mines for 1819, which is stated to contain the latest observations of the Abbé Haüy on that substance.

A list is given in this memoir of the values of 31 angles, primary and secondary; and as far as I have compared them with the measurements I have taken on the natural planes of some brilliant crystals, and on some planes obtained by cleavage, not one of them is correct.

The form assumed in this memoir as the primary one is an octahedron with a rhomboid, or oblique angled parallelogram, for the common base of the two pyramids; and for the sake of rendering the position of some of the measured planes apparent to the reader, I have given this figure, although I do not concur in the necessity of adopting this as the primary form, having observed a cleavage parallel to the edge C, and to a line joining the summits E and E' of the octahedron.



The following are the values of some of the angles as given by M. Cordier contrasted with those afforded by the reflective goniometer:

Cordier.				Reflective Goniometer.			
P on P' $116^{\circ} 44'$ ....				$121^{\circ} 00'$ on the natural planes or cleavages of three crystals.			
				121 03 on a cleavage by Mr. Phillips.			
P	M	127	32	....	125	25	
P	M'	82	14	....	81	35	
P	h	121	38	....	119	30	h is a plane parallel to the edge c.
M	M'	97	46	....	98	30	



It is unnecessary to add to this list of differences, as these render the errors sufficiently apparent. Whence they arise, I am at a loss to conjecture; but they are considerable enough to be detected upon even imperfect specimens by the common goniometer.

H. J. BROOKE.

## ARTICLE IX.

### ANALYSES OF BOOKS.

- A Critical Examination of the first Principles of Geology; in a Series of Essays.* By G. B. Greenough, President of the Geological Society, F.R.S. F.L.S. London.

(Concluded from p. 373.)

#### III. *On the Inequalities which existed on the Surface of the Earth previously to diluvian Action, and on the Causes of these Inequalities.*

In the second essay the author gave us his opinions relative to the causes of the inequalities which diversify the present surface of the earth; but as the operation of these causes must have been greatly modified by the form of the surface on which they acted, he has thought it requisite in the third essay to inquire into the figure of the earth before the deluge, to which in the preceding essay he ascribed the present inequalities of the surface.

Was the antediluvian earth a level plain, or was it like the present, diversified with mountains and valleys?

Stracey, Hutchinson, and many of the earlier writers, adopted the former opinion; but our author thinks that it is untenable. The mixture of the fossil remains of sea animals and wood in the secondary rocks demonstrates that the earth at the time of the formation of these rocks was partly sea and partly dry land, as at present. Hence it must have been diversified into mountain and valley.

The antediluvian earth then was uneven in its surface, as is the case at present. To what must we ascribe this inequality? Our author assigns four causes.

1. *Crystallization*.—By this term is usually meant the regular shape into which the integrant particles of various substances arrange themselves, in consequence, as is supposed, of a certain polarity with which they are endowed. Thus the integrant particles of alum form regular octahedrons, those of common salt cubes, &c. Prof. Jameson supposes the earth to be crystallized in this sense, and the different strata to be crystalline laminæ. But this notion, which seems to have been taken up without due reflection, is at variance with every principle of crystallo-



graphy and geology, and is not surely entitled to a serious examination or refutation.

Rouelle and Lametherie were of opinion that rocks exhibiting a foliated, radiated, or fibrous texture have been formed by irregular crystallization into groups insulated at their summits, but united at their bases. This opinion is very probable. Our author conceives that in such cases the particles have associated without any regard to polarity; but I apprehend that this notion is not quite correct. If we suppose the particles of a body to be possessed of poles, we must mean that at one extremity they have the property of repelling each other, while their other extremities have the property of attracting each other. This is what is meant by *having poles*, and the notion is borrowed from the phenomena of common magnets. Now if the particles of a body possess polarity, it is obvious that they can unite with each other only in one way; so that irregular crystals and regular crystals are formed precisely in the same way. The only difference between them is, that in the one case we perceive the regular shape of the crystal; while in the other case that shape is concealed. It is quite obvious that in calcareous spar the particles of the carbonate of lime are arranged precisely in the same order as in the most regular crystals of limestone; for the fragments are all rhomboidal, and exhibit the very angles which exist in the primitive crystal of that mineral. I believe this to be the case in all foliated minerals. Indeed Mr. Daniel has shown it to be so in a variety of unexpected instances. The reason why the regular shape does not appear to the eye is the particular state in which the crystallization has taken place. When a liquid matter becomes rapidly solid, or when it fills the whole space in which it exists, it is impossible for the regular form to appear to the eye; because the different crystals are entangled in each other, and fill up all the interstices between each other. Radiated and fibrous minerals are in the same circumstances as foliated minerals. The only difference between them is in the breadth of the plates. When the plates are as broad as the mineral, it is said to be foliated; when several plates are required to make up the breadth, it is said to be radiated; and when the breadth of each plate is very small compared to its length, it is said to be fibrous. Hence the observations made with regard to foliated minerals apply likewise to radiated and fibrous minerals.

With regard to Delametherie's notion, that sandstone, chalk, plants, &c. are crystallized, it is not necessary to say any thing. When he applied the term to these substances, it is obvious that he affixed to it a meaning different from that which it commonly bears. He seems to have considered chemical affinity, or attraction of cohesion, to be all that was necessary to constitute a crystal; but a crystal in common language always is a substance formed into a regular shape, and of course composed of

integrant particles, having all of them the same shape, and united in the same way. It is obvious that fragments of quartz, &c. can never form a crystal, because they cannot possess the same shape, and they must of necessity be destitute of polarity.

2. *Partial Deposition*.—Our author supposes that all the materials that constitute the strata which cover the surface of the earth were in solution in a fluid, and that this fluid deposited more of the matter held in solution over one place than over another. This would occasion inequalities in the new surface formed. He ascribes these partial depositions to the action of tides and currents.

3. *Subsidence*.—After the strata were deposited, they often gave way, and sunk either by shrinking, or by the pressure of the strata deposited over them. The inequality of this subsidence would occasion heights and hollows on the surface of the earth. Every case of subsidence implies a *fault*; but faults may exist without subsidence; for if new matter were deposited at present on the surface, it is obvious that the strata could not be on a level owing to the inequalities existing before this deposition.

4. *Volcanoes and Earthquakes*.—The effects of these our author thinks would be the same as at present. They would form occasional hills by the accumulation of ashes or scoria, or valleys, by the falling in of unsupported craters.

But the action of running water, he thinks, has been in all ages the principal cause of inequality of surface.

#### IV. Of Formations.

By *formation* is meant a series of rocks supposed to have been formed in the same manner and at the same period. The term was introduced by Werner, and is purely theoretical. When two substances are intermixed with each other, or when they alternate with each other, they are supposed to have a common antiquity and origin. But our author is of opinion that neither of these circumstances affords a proof of a common origin. I do not see, however, how two different substances could be found intermixed together, unless they had been deposited at the same time. Thus granite is a mixture of felspar, quartz, and mica. How is it possible to avoid concluding that these three ingredients were deposited at the same time? In the valley of the Pentlands, near Edinburgh, we find a vast number of thin beds of greywacke and greywacke slate alternating with each other. How is it possible to avoid believing that these beds were deposited nearly at the same time? These beds stand nearly perpendicular, and they are covered by beds of greenstone, compact felspar, &c. lying nearly horizontal. How can we avoid concluding that these horizontal beds were deposited at a different time from the perpendicular beds on which they lie?

Werner and his followers are of opinion that formations are *universal*, or that they extend (though with interruptions) round

the whole globe of the earth. Our author controverts this doctrine; and it must be admitted that it stands upon very slight evidence; but I conceive that the assertion that formations are universal means nothing more than that rocks occur in the *same* order in *all* countries. That certain rocks are always nearest the centre of the earth (in position), and certain rocks always nearest the surface. Now the facts brought forward by our author to controvert this opinion prove only that certain members of the series are wanting in different countries—a fact which has been universally admitted; but I do not see how this can overturn the opinion, that rocks in every country affect a particular order of arrangement. But the discussion of this point comes more naturally under Essay V. which is entitled

*On the Order of Succession in Rocks.*

Our author in this essay controverts the Wernerian doctrine, that rocks follow a determinate order, and seems to affirm that the order is perfectly capricious, and that from the arrangement of rocks in one country, we can draw no inference with respect to their arrangement in any other country. If there be no regularity in the arrangement of rocks; if they follow no particular order; if we can form no notion from the rocks which we observe, whether any particular rock is likely to occur or not, then the science of mining is founded upon no legitimate data, and there can be no difference between the chance of success when the most skilful and the most ignorant man are sent to survey a country. A little consideration I should think must be sufficient to satisfy our author that he has carried his scepticism on this subject too far; and that while anxious only to get rid of the weak and rotten parts of the structure, he has in reality demolished the whole science of geology; for if there be no regularity in the position of rocks; if every thing in the structure of the earth is perpetually varying in the most capricious manner, then geology can have no foundation whatever; we can acquire no general knowledge from an examination of the structure of the globe, and the science must be considered as on a level with astrology and necromancy. Would our author expect to meet with beds of coal in a country composed of granite or mica slate? Would he expect to meet with granite in a coal country? Would he search for mines of tin among rocks composed of shell limestone? If he found blende, would he not be induced to look for accompanying galena? These, and many other examples, which will immediately occur to every one conversant with the subject, are sufficient to show us that the position of rocks is not quite so capricious as our author supposes; that certain substances have a tendency to associate together, and that mining and surveying are not without their laws and their rules.

It may be very true that Werner generalized too rapidly; that Saxony and Bohemia were too small a portion of the earth to



render it safe for him to draw general inferences from the order of position which he there observed. It may be true that he was not very successful in his attempts to ascertain the order of the rocks of which these countries are composed—all this and more may be true, and yet Werner may have been fortunate enough to hit upon a fundamental geological fact upon the prosecution of which the future progress of the science may in a great measure depend.

As far as it is possible to judge of Werner from the few writings which he has left us, his powers of arrangement seem to have been rather deficient, and he displays a want of taste and a minuteness in his subdivisions which render it peculiarly disagreeable to peruse his writings. He probably possessed that enthusiasm for his profession, and that peculiar kind of eloquence, which captivates young minds; for his reputation was chiefly owing to the panegyrics of his scholars, almost all of whom viewed him with a mixture of love and admiration. In his geological speculations, he attempted more than it was possible for one man to accomplish. Hence his inferences are frequently such as must be relinquished when we judge of them by a comparison of the structure of different countries with his geognosy. At the same time it is but fair to remark, that Werner never published any thing on the subject of geognosy, that his opinions were merely stated in his lectures, and afterwards given to the world by his pupils from what they recollected of the peculiar opinions of their master. Now every Professor who has had an opportunity of looking at the notes of his lectures taken by his students must be aware how extremely apt they are to misrepresent his meaning, or even to ascribe to him sentiments different from those which he really entertains. I think I can perceive traces of this misrepresentation in the Wernerian geognosy, as retailed to us by the most distinguished of Werner's pupils. For example, there is a mountain of a very remarkable character which occurs in the primitive country of Saxony, to which the name of *topaz rock* was given, because it contained that beautiful stone. It was natural for Werner to call the attention of his students to this remarkable rock. This he did by placing it provisionally among the primitive formations, not, I am persuaded, as a thing already established, but as a doubtful point to be decided by future and more extensive observations. Those pupils who published the outline of the Wernerian geognosy omitted the mark of hesitation with which Werner probably spoke of this rock, and reckoned it among the established general primitive formations. Thus they introduced an absurdity into the science of which Werner was, in all probability, not guilty; and such is the veneration with which the Wernerians are in the habit of viewing the statements of their master that no writer among them (at least so far as I know) has ventured to strike out the *topaz rock* from the list of general

primitive formations ; though there can be no doubt that it is a mere variety, or rather deviation, from another formation under which it should be placed.

The trap rocks in Germany are so imperfectly exposed that it was very difficult in that country to form accurate conclusions respecting their structure and position. This accounts for the mistakes into which Werner seems to have fallen respecting them. The same observation applies to the floetz rocks. Great Britain is much better adapted for the study of these rocks than Germany. In the middle districts of Scotland, the trap rocks occur in vast abundance, and so fully exposed that they afford the greatest facilities for examining their structure and position. The same may be said of the coal formation which at Crossfell and some other places is most admirably laid open for examination.

It seems to be true that the primitive formations alternate with each other differently in different places ; though several of the examples brought forward by our author are, I believe, inaccurate. The structure of the Grampians, for example, is represented as quite capricious ; yet, as far as I have examined it, we find a great conformity, with the exception of those parts where deposits of porphyry occur ; for this rock where it makes its appearance, as at Glenco and in Cumberland, renders it very difficult for us to make out the structure of the country.

But there is a striking distinction between rocks which was first pointed out by Lehnian. Some rocks contain no traces of animal or vegetable remains. We have, therefore, no evidence that they have not existed since the original formation of the globe. There are other rocks which contain these remains. These must have been formed after the earth was inhabited by animals and vegetables. They are not, therefore, original, but secondary. They consist chiefly of fragments of the other rocks, indicating that they were formed by the destruction of part of these rocks, and that they involved with them the remains of the animals and vegetables which at that time existed. Now it is a matter of fact that the first of these rocks, or the *primitive*, as they have been called, lie always (or almost always) below the secondary, or have the beds of the secondary rocks reposing against their sides. Here then is a regular arrangement of rocks, clear, obvious, and undisputable. The primitive come first in order, and they are succeeded by the secondary.

It is true that one or two examples occur, as at Christiania, in Norway, if Von Buch's observations be correct, where primitive rocks are observed lying over secondary rocks, or rocks containing animal fossils. Such cases should be carefully described and examined as *exceptions* to the general order of the arrangement of rocks. When the degree of rarity of the occurrence of such examples has been ascertained, and when all the phenomena have been accurately examined, perhaps, it will be in our power



to account for the seeming anomaly from some changes which have taken place in the position of these rocks in consequence of a catastrophe posterior to their formation. They are not more extraordinary than the nature and position of some of the floetz rocks described by Mr. Webster in the Isle of Wight. Such rare deviations ought not to be considered as overturning the general order which the rocks follow with respect to their order.

Werner's *transition* rocks certainly are entitled to attention in consequence of their very peculiar nature. His arrangement, whether accurate or not, has been attended with the advantage of calling the attention of geologists to these rocks. We find them always grouped together. They sometimes contain fossil remains of animals and vegetables, and sometimes not. In the former case, as far as my own observations go, they are associated with floetz rocks; in the latter case, with primitive rocks. Greywacke, which constitutes the most striking and characteristic of the transition rocks, differs a good deal in its appearance, and I have been for some time of opinion that two distinct rocks have been confounded together under the same name. I believe that a greywacke exists to which the epithet *primitive* may be applied, as it never contains fossils, or alternates with rocks that contain fossils; while there is another variety that associates with floetz rocks, and contains fossils. It alone is entitled to the epithet *transition*, or *floetz*, if our author chooses to discard the term *transition* altogether.

I agree with our author that Werner's term *transition* implies an absurdity. Werner, or his pupils (I know not which), attempted a great deal too much when they undertook to detail the order of the formation of all the rocks constituting the crust of the earth. Indeed their repeated inundations and retreats of the waters may be considered as perfectly on a par with Burnet's account of the antediluvian world, and the effects of the deluge. All we know about the rocks composing the crust of the earth is, that some are destitute of fossils, while others contain them. The first are *original* rocks, about the formation of which it seems folly to speculate. The second set must have been formed after the earth was inhabited by animals and vegetables; though, as human remains have not been observed in them, probably before the existence of man. Unless we suppose, as has been done, that the secondary rocks were formed by a deluge, and that after this deluge, sea and dry land changed places; so that the antediluvians and all their works were buried under the bottom of our present ocean. The secondary rocks are formed of the debris of the primitive, and lie over them, except in some rare cases, to be accounted for by earthquakes or other similar catastrophes. To suppose a set of rocks formed while the earth was passing from a state of not being capable of supporting animals and vegetables to a state capable of supporting them is, to say the least of it, a most whimsical and unlikely supposition.

It would be too much to suppose that Werner's arrangement of the floetz rocks, which he himself admitted to be very imperfect, is rigidly accurate; yet when we compare it with the structure of England, the agreements are much greater than we could have expected *à priori*. Some of Werner's floetz formations, indeed, as his gypsums, either do not exist in England, or they constitute beds of trifling size and extent; while some, as the oolite, occupy a much greater space in England than they seem to do in Saxony and Bohemia, from which Werner seems to have drawn his notions. But whoever makes a comparison of the Wernerian floetz rocks with Mr. Smith's geological map of England, or with Mr. Buckland's table of the order of the strata of England and Wales, will be struck with the resemblance.

I do not see the force of our author's refutation of the Wernerian assumption, that *granite* is the fundamental or lowest rock.

With respect to the specific gravity of rocks, though it is not, strictly speaking, true, that the rocks which constitute the surface of our earth are deposited accurately according to their relative specific gravities; yet when we allude to the structure of the whole globe, it is obvious that the concentric shells of which the globe is composed must increase in specific gravity as they approach the centre. This would be the case even supposing every shell composed of the same kind of rock. The reason is, that every shell is subjected to the pressure of all the shells between itself and the surface of the earth; so that the specific gravity of each shell must increase in a geometric ratio as it approaches the centre of the earth. Let us suppose the earth composed of a million of concentric shells, inclosed within each other like the coats of an onion. Let the specific gravity of the outermost shell be  $2\frac{1}{4}$ , that of water being 1; then it has been demonstrated that the specific gravity of the stratum forming the millionth shell, or the nucleus of the globe, supposing it merely increased by the pressure of the shells lying over it, would exceed  $13\frac{1}{4}$ . It is obvious from this that we cannot reason about the nature of the strata which constitute the interior of the earth; because what materials soever we suppose them composed of, the specific gravity would increase geometrically as they approach the centre.

The work, which I have been analyzing, contains three more essays, with the following titles:

VI. *On the Properties of Rocks, as connected with their respective Ages.*

VII. *On the History of Strata, as deduced from their Fossil Contents.*

VIII. *On Mineral Veins.*

These essays contain much information, and many judicious observations, in the justice of which I heartily concur. But I

could not enter into an analysis of them without occupying another number of the *Annals*. As many of my readers would probably consider this as exceeding the requisite allowance, I must satisfy myself with referring those readers who are interested in the subject to the work itself, from the perusal of which I can promise them much information and considerable entertainment.

## ARTICLE X.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

On Nov. 4, this Society recommenced its sittings; when the Croonian lecture, by Sir E. Home, was begun. It was entitled "A Further Investigation of the component Parts of the Blood."

Nov. 11.—The Croonian lecture was concluded. The author attempted to show that globules are to be found in the blood, of a less size, and of a different nature, from those commonly supposed to exist in that fluid. These were first observed by Mr. Bauer during an examination of the layers composing an aneurismal tumour. In the coat in contact with the circulating blood, these smaller globules were observed in the proportion of 1 to 4 compared with the larger globules, but in the other layers they were more numerous; and in that which had been first formed, they existed in the proportion of 4 to 1. Their size was estimated by Mr. B. at  $\frac{1}{8000}$ th of an inch. In making a section of another aneurismal tumour, crystals of sulphate of lime, and muriate and phosphate of soda, were observed, which salts, as well as the globules above-mentioned, Sir E. supposed to have originally existed in solution in the serum, the globules being only to be seen after the coagulation of the blood had taken place.

In the coagulated lymph formed during violent inflammation, the same globules were observed mixed with a few colourless blood globules. In the upper and firmest coat of the buff of blood, they were likewise very numerous, but the lower and softer parts were found to be composed chiefly of blood globules. To distinguish these globules from the larger blood globules, the author proposed to call them globules of *lymph*.

The author proceeded to state that the quantity of carbonic acid gas evolved under the exhausted receiver of an air-pump from buffy blood was much less than that from healthy blood, but that by far the greatest proportion of this gas escaped from the blood of a healthy person drawn an hour after a full meal.

Mr. Bauer found both lymph and blood globules in the mucus of the pylorus and duodenum. In the chyle, he found the size of the globules various. Mr. B. supposes that the blood globules

are formed in the mesenteric glands, with the exception of the colouring matter, which, he thinks, they acquire on exposure to air in passing through the lungs.

At this meeting the Bakerian lecture by Mr. Brande was begun: "On the Composition and Analysis of the Inflammable Gaseous Compounds resulting from the destructive Distillation of Coal and Oil, with some Remarks on their relative heating and illuminating Powers."

Nov. 18.—The Bakerian lecture was concluded. In the first part of this lecture, the author attempted to show that no other compound of carbon and hydrogen can be demonstrated to exist than what is usually denominated olefiant gas, consisting of one proportion of carbon and one of hydrogen; and that what has been usually termed carburetted hydrogen is in reality nothing but a mixture of hydrogen and olefiant gases. In proof of this opinion a series of experiments was detailed, made upon gaseous products obtained from coal, oil, and other substances, and in various ways, the results of all which tended to establish the truth of the above opinion.

The author advanced the supposition that many of the products usually obtained by the destructive distillation of coals, &c. are of secondary formation; that is to say, that they result from the mutual action of the first formed gases at high temperatures. Thus a peculiar compound of hydrogen and carbon was stated to be formed by passing pure olefiant gas through a tube containing red-hot charcoal. This substance was similar to tar in appearance, but possessed the properties of a resin. So also by the mutual action of sulphuretted and carburetted hydrogen, sulphuret of carbon was stated to be formed. In this part of the lecture some new modes of analyzing gaseous mixtures were pointed out.

In the second section, comparative experiments were detailed on the illuminating and heating powers of gases from coal and oil. The general results were, that the illuminating powers of olefiant, oil, and coal gases, are to one another nearly as 3, 2, and 1, and that the ratio of their heating powers is nearly similar; that is to say, that more heat is produced by the gas from coal than by that from oil, and by the gas from oil than by olefiant gas. In this part of the lecture was also strikingly illustrated by experiments the great advantage obtained in point of illuminating power, by forming the burners of many jets, in preference to a single one, especially when the jets are made so near to one another that the different flames can unite.

The lecture was concluded by some comparative experiments on the properties of terrestrial and solar light. The light produced by gases, even when concentrated so as to produce a sensible degree of heat, was found to occasion no change in the colour of muriate of silver, nor upon a mixture of chlorine and hydrogen gases; while, on the other hand, the concentrated



brilliant light emitted from charcoal, when submitted to galvanic action, not only speedily affected the muriate of silver, but readily caused the above gaseous mixture to unite, sometimes silently, and often with explosion. The concentrated light of the moon, like that from the gases, did not affect either of these tests. The author in conclusion remarked, that having found the photometer of Mr. Leslie ineffectual in these experiments, he employed one filled with the vapour of ether (renewable from a column of that fluid), and which he found more delicate.

At this meeting, a paper, by Dr. Carson, was begun, "On the Elasticity of the Lungs."

## ARTICLE XI.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Arsenic.*

Arsenic is one of the simple bodies which has not yet been reconciled in a satisfactory manner to the atomic theory. In the last edition of my *System of Chemistry*, I deduced the weight of an atom of it, from the best experiments hitherto made on the subject, to be 4.75. I concluded likewise that the oxygen in arsenious and arsenic acids were to each other as the numbers 3 to 5, and that their constituents were as follows :

Arsenious acid. . .	100 arsenic	+ 31.600 oxygen
Arsenic acid. ....	100	+ 52.631

This makes the numbers representing the atoms of arsenic and oxygen united in these bodies as follows :

Arsenious acid . . . . .	4.75 arsenic	+ 1.5 oxygen
Arsenic acid. . . . .	4.75	+ 2.5

or the first is a compound of 1 atom arsenic + 1.5 atom oxygen, and the second of 1 atom arsenic + 2.5 atoms oxygen. It is obviously possible to get rid of these fractional parts of atoms by multiplying the preceding numbers by 2. This will give us 9.5 for the weight of an atom of arsenic, and arsenious acid will be composed of 9.5 arsenic + 3 oxygen, and arsenic acid of 9.5 arsenic + 5 oxygen. An atom of arsenious acid will weigh 12.5, and an atom of arsenic acid 14.5. I thought it premature to propose this view of the subject till further experiments should be made on the composition of arsenious acid.

Berzelius has since published a new set of experiments to determine the composition of both these acids, and the proportions in which they unite with bases. These experiments I



intend to insert in an early number of the *Annals of Philosophy*. At present I shall merely observe that Berzelius has adopted my view of the subject, which his experiments seem to me to establish in a satisfactory manner. I have been a good deal gratified to perceive that all the new determinations that have been made since the publication of my last edition approach nearer than the former ones to the numbers which I have pitched on for the weight of the atoms of bodies.

According to Berzelius's new analyses, these two acids are composed as follows :

Arsenious acid. . .	100 arsenic	+ 31.907 oxygen
Arsenic acid. ....	100	+ 53.179

numbers which almost coincide with those which I have already given, partly from my own experiments, and partly from those of Proust.

Berzelius found arseniate of lead composed of

Arsenic acid ..	34.14	....	100.00	....	7.257
Oxide of lead ..	65.86	....	192.91	....	14.000

I have added the last column in the preceding table, from which we see that if 14 be the equivalent number for protoxide of lead, as I have shown it to be, then the equivalent number for arsenic acid is 7.257. Now 7.25 is the number which I assigned for arsenic acid in my System ; therefore I consider the analysis of Berzelius as corresponding exactly with my previous views.

Berzelius formed two arsenites of lead. The first, by precipitating nitrate of lead by arsenite of ammonia ; the second, by mixing together arsenite of ammonia and subacetate of lead. The first of these he found composed as follows :

Arsenious acid . . .	47.356	....	100.00	....	12.593
Protoxide of lead ..	52.644	....	111.17	....	14.000

The composition of the second was as follows :

Arsenious acid .....	100.0	.....	6.377
Protoxide of lead .....	219.5	.....	14.000

It is quite obvious that the first of these salts contained twice as much arsenious acid as the second. If we consider the second as composed of one atom acid and one atom base, then the first must be a compound of two atoms acid and one atom base. On this supposition, the equivalent number for arsenious acid will be :

By first salt .....	6.296
By second salt .....	6.377
Mean. ....	6.336

The number which I had chosen is 6.25. Now I consider

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Berzelius's analyses as a full confirmation of the accuracy of this number; for it differs less from the number given by the first analysis than the two analyses do from each other.

It seems then established that the numbers for these two acids are as follows :

Arsenious acid. ....	6.25
Arsenic acid .....	7.25

If any person wishes to get rid of the anomaly of the half atom, he has only to double these numbers by supposing an atom of arsenic to weigh 9.5. On such a supposition, the arseniate of lead analyzed by Berzelius must be a compound of one atom arsenic acid + two atoms protoxide of lead. The first arsenite of lead which he analysed will be a compound of one atom arsenious acid and one atom protoxide of lead; while the second contains one atom acid + two atoms base.

This supposition, which is the simplest, may very likely be true; but it will be necessary before we adopt it to analyze a greater number of the arseniates than have hitherto been subjected to an experimental investigation. My object here is merely to show that my previous views are fully confirmed by Berzelius's experiments; and to inform my readers that he has given up his previous notions respecting these acids, which indeed were untenable.

## II. *Calculus from the Bladder of a gouty Person.*

Some weeks ago a medical friend of mine in Birmingham sent me a small specimen of calculous matter found after death in the bladder of a man of 64 years of age who had been afflicted with gout for 10 years.

The substance was greyish-white, and had a good deal the aspect of bone earth. It was partly in powder, and partly in small nodules, that appeared to be formed of concentric layers, and which were rather hard. Considering it from its appearance as bone earth, I digested a little of it in muriatic acid; but did not find it sensibly to diminish. The muriatic acid being poured off, and evaporated to dryness in a watch-glass, left a slight saline crust, which from the taste, solubility, and the action of oxalate of ammonia on it, I concluded to be muriate of lime.

Another portion of the calculus being put into nitric acid and heated, dissolved with effervescence, and the solution, which was colourless, being evaporated to dryness, left a beautiful pink sediment. Hence the calculus was chiefly composed of uric acid.

One grain of it was exposed for an hour to a red heat in a muffle, being placed in a platinum cup. It was now reduced to 0.1 gr. The residuum was white, and resembled bone ashes. Water being digested on it acquired the property of giving a purple tinge to paper stained red with cudbear, indicating the

presence of an alkali. The water being evaporated to dryness left a white crust on the glass, which was tasteless, insoluble in water, but soluble with effervescence in nitric acid, and the solution was precipitated white by oxalate of ammonia. It was, therefore, carbonate of lime; so that the alkaline properties of the water were derived from some lime which it held in solution.

The residual matter dissolved without effervescence in muriatic acid, and was precipitated white by ammonia. It was, therefore, phosphate of lime.

Thus the calculus was composed of

Uric acid,  
Phosphate of lime,  
Lime.

I think it not unlikely that the lime had been in combination with the uric acid; but the quantity of calculus (amounting only to three or four grains) was too small to enable me to determine the point by a more rigid analysis.

The fact, however, of pure lime being separable by muriatic acid, and by water after simple exposure to a red heat, I think of some importance. I am not aware that it has been hitherto observed.

III. *Query respecting the Method of coating Metals with Platinum.* By T. Howse, Sen.

(To Dr. Thomson.)

SIR,

Cirencester, Sept. 27, 1819.

From some directions for washing metals by means of amalgams, in Rees's Cyclopædia, I was led to hope that it would be a matter of no great difficulty to apply the valuable metal platinum to some of the common purposes of life. According to those directions, I have precipitated platinum from its solution by adding muriate of ammonia, heated the precipitate to expel the acid and oxygen, and afterward heated it again with the addition of mercury, to form an amalgam, for the purpose of coating brass or copper, from which the mercury was to be driven off by heat applied to the article coated with the amalgam. I have tried several variations of the process, but hitherto without success; I should, therefore, feel obliged if you would furnish me with the method actually practised, if it be known, through the medium of your *Annals*. I fear that a covering of platinum laid on by means of mercury, must, if practicable, be so thin as to possess little durability. A chemical friend informs me that copper articles plated with platinum, the two metals being in the proportion of 15 to 1, may be purchased of M. Labouté, rue Neuve-Saint-Eustache, No. 4, at Paris, at about 37s. per pound; but he is not aware that this method of plating is yet published.

I remain, Sir, your constant reader,

T. Howse,

\* \* I am not aware that any process for covering metals with platinum has been hitherto published, but have little doubt, from some trials that I have made, that the process followed for gilding by means of the amalgam of gold would answer likewise with platinum. A great deal depends upon the affinity of the metal for mercury. Of course it will be more difficult to succeed on steel than on copper. On brass the process should be easy. The surface of the brass should be in the first place amalgamated with mercury, and much will depend upon the state of the amalgam. If my correspondent would take the trouble to visit some of the gilding manufactories at Birmingham, for example, he would probably meet with a solution of most of his difficulties. In some cases, the method of M. Leithner, described in the *Annals of Philosophy*, v. 20, might, perhaps, be practised with advantage.—T.

#### IV. *On the Alloy of Platinum and Tin.* By Dr. Clarke.

(To Dr. Thomson.)

DEAR SIR,

In the number of your *Annals* for *September* last (vol. xiv. No. iii. p. 229), there is a communication from me upon the subject of an experiment alluded to by one of your former correspondents. It is that which relates to the astonishing affinity of *platinum* and *tin* at no very exalted temperature before the common blow-pipe. By some unaccountable mistake, the word *lead* has been substituted for that of *tin*. By reference to the notice of your correspondent Mr. Fox, (p. 467, vol. xiii.) you will perceive that it never was my intention to describe any other effect than that produced by the fusion of *platinum* and *tin*. Throughout the whole of my late communication, therefore, for *lead* and *lead-foil* read *tin* and *tin-foil*. The effect of melting *platinum* and *lead* together is well known; the two metals unite tranquilly together; but when *tin* is substituted for *lead*, the effect takes place which I have described; and the light and heat emitted are truly surprising.

E. D. CLARKE.

#### V. *Chemical Chair at Berlin.*

The chemical chair at Berlin, left vacant by the death of Prof. Klaproth, is still unoccupied, Prof. Berzelius and Prof. Gmelin, of Heidelberg, having declined filling it. A young man of some talents, who is to follow Berzelius to Stockholm, and to study for some years under his auspices, is spoken of as destined to become Professor of Chemistry.

#### VI. *Water of the Dead Sea.*

The water of the Dead Sea has attracted the attention of chemists in consequence of the great quantity of salt which it contains. A very elaborate and, I believe, accurate analysis of it was published a good many years ago by Dr. Marcet in the



**Philosophical Transactions.** Afterwards it was analyzed again by Prof. Klaproth. The result of his experiments, and the animadversions on them by Dr. Marcet, may be seen in the first volume of the *Annals of Philosophy*. A few months ago, Gay-Lussac published another analysis of the same water. The specimen examined was brought by the Comte de Forbin. I shall exhibit the results of these three analyses in the following table. The saline contents were extracted from 100 of water.

	Marcet.		Klaproth.		Gay-Lussac.
Common salt. ....	10·676	....	7·80	....	6·95
Muriate of lime. ....	3·792	....	10·60	....	3·98
Muriate of magnesia. ....	10·100	....	24·20	....	15·31
Sulphate of lime. ....	0·054	....	—	....	—
	<hr/> 24·622		<hr/> 42·60		<hr/> 26·24

The specific gravity of the water examined by these gentlemen differed somewhat; depending, perhaps, upon the season of the year in which the specimens were taken. The following table exhibits the specific gravities as stated by each of them:

Dr. Marcet . . . . .	1·211
Prof. Klaproth. . . . .	1·245
M. Gay-Lussac . . . . .	1·2283 at 62°

I think it probable that the greater specific gravity of the water examined by Gay-Lussac, together with his making an allowance for the muriatic acid driven off during the exposure of the muriate of magnesia to a red heat, is the cause of the greater proportion of salt found by him in this water than by Dr. Marcet. The method of determining the true quantity of salts contained in water may, at first sight, appear very easy, but it is a problem attended with considerable difficulty. The method which I find to answer best is to put a given weight of the water into a Florence flask, the weight of which is noted, to incline the flask upon a sand-bath, and to evaporate the water away. I then expose the bottom of the flask to a red heat, and weigh it when cold. From this it is easy to deduce the weight of the saline contents, provided care be taken to add the proportion of muriatic acid which may be driven off from various earthy muriates (especially muriate of magnesia), if they are present.

Klaproth's salts were not dried at a red heat. This accounts for the great weight of the salts which he obtained; and makes it difficult to draw any accurate conclusions from his analysis. The muriate of lime found by Marcet and Gay-Lussac nearly agree. We find the same approach to coincidence in the quantity of common salt and muriate of magnesia taken together; but Dr. Marcet found a much greater proportion of common salt than M. Gay-Lussac; while, on the other hand, Gay-Lussac



found a much greater proportion of muriate of magnesia than Marcet. It is difficult to say on which side the error lies. In consequence of the great care which Dr. Marcet took, and the number of times that his analysis was repeated, I rather feel disposed at present to prefer his results; but another analysis by some other person, with all the precision that our present analytical methods admit, will be necessary before the point can be considered as settled.

Gay-Lussac found traces of muriate of potash in this water. He exposed it to a temperature of  $19^{\circ}$  without any crystallization taking place. Hence it follows that the water of the Dead Sea is not saturated with salt.

### VII. *Beautiful luminous Phenomenon.*

(To Dr. Thomson.)

SIR,

Newton-Stewart, Oct. 18, 1819.

A singular and beautiful phenomenon appeared in our atmosphere here last night about eight o'clock; it was a *bow, or arch, of silvery light*, stretching from east to west, and intersecting the hemisphere at a few degrees to the southward of the zenith; its breadth varied considerably during its continuance; but when in its most perfect and defined state might have formed with the eye an angle equal to what the breadth of the rainbow generally does. The intensity of its light might be compared to that of a thin cloud illumined at night by the moon, and was greatest towards the extremities of the arch, both of which were lost in dark clouds. I was not an eye-witness to its formation, but was told it first shot up from the eastern horizon. After it had remained very bright for 20 minutes, or so, dark blanks were first observed to take place here and there; then after expanding a little in breadth, and shifting for a short way further to the southward, it disappeared. Some time before its appearance, the atmosphere had been very cloudy; but when it was formed, the sky was free of clouds, except towards the horizon, to the westward and northward, where they hung very dark and heavy. It was also a calm, but the wind had been at N. the barometer stood high, and the thermometer at  $43^{\circ}$ . To account for such a singular phenomenon, I make no pretensions; but it was strikingly different from any of the usual forms of the boreal lights, *which too* were seen very vivid in the course of the evening. From its steady permanence for such a long time, the body illumined could be recognised as of a *vapory nature*, and the stars were discernible through its medium at several places.

The above appearance was observed with a great degree of interest and admiration by all who saw it; and I have transmitted you this notice of it, judging that it will not be unacceptable to you, or uninteresting to some of the readers of your *Annals of Philosophy*.

I am, Sir, with respect,

B.

VIII. *Boracic Acid in Tuscany.*

Mineralogists have been for some time aware of the existence of boracic acid in solution in certain lakes of Tuscany. M. Robiquet informs us (*Journal de Pharmacie*, 1819, p. 261) that he was told by M. Dubrouzet, one of the proprietors of the lakes of Cherchaio, that he obtained about two per cent. of the acid from the waters subjected to evaporation. M. Robiquet was offered any quantity of this acid delivered in Paris at the price of three francs the kilogramme. It would appear that considerable quantities of it had been sent to Paris; but no purchasers have hitherto presented themselves.

From the trials made by M. Robiquet with this acid, we learn that it is in small scales, of a greyish colour. Its taste is slightly bitter. Its solution in water reddens litmus; it is not precipitated by nitrate of silver, nor oxalate of ammonia; but strongly by muriate of barytes. It contains, therefore, an alkaline sulphate mixed with it. When dissolved in water, it left an insoluble residue on which the mineral acids had no action. It was composed of different earthy bodies mixed with a little oxide of copper.

M. Robiquet suggests the propriety of adding the requisite quantity of soda to convert this acid into borax; and if any confidence can be put in the calculations which he makes, the speculation would be attended with considerable profit.

IX. *Sagacity of a Newfoundland Dog.*

I am tempted to copy the following anecdote of a Newfoundland dog from Anspach's *History of the Island of Newfoundland*, just published. I have seen the story before; but shall not attempt to determine whether the original relator of the story, or Anspach, be the authentic account; or whether the same thing may not have happened with two different dogs.

"The last quadruped that we shall mention under this head, though very far from being the least in worth, is the Newfoundland dog, a valuable and faithful friend to man, and an implacable enemy to sheep. When born or reared from an early age under the roof of man, this dog is the most useful animal in the island as a domestic. He answers some of the essential purposes of a horse, is docile, capable of strong attachment, and easy to please in the quality of his food; he will live upon scraps of boiled fish, whether salted or fresh, and on boiled potatoes and cabbage; but, if hungry, he will not scruple to steal a salmon, or a piece of raw salt pork from the tub in which they have been left to steep; he is likewise fond of poultry of the larger kind; but, as a beverage, nothing is equal in his estimation to the blood of sheep. The author had purchased a puppy of the true breed, which had been brought from the northward of the island to Harbour-Grace. This puppy grew up to the size of a small donkey, as strong and fit for hard work, as he was tractable and gentle, even with the children of the family, of whom he seemed

to be particularly fond; nor was he ever known, in any one instance, to disagree with the cats of the house, whom he treated rather with a kind of dignified condescension. But the *dog*, unless closely watched, would run after sheep wherever he could trace them, even drive them from high cliffs into the water, and jump in after them; not, however, without first considering the elevation of the cliff; for, if he thought it too great, he would run down and take the nearest more convenient place to continue his pursuit. The owner of that dog had, at one time, some domesticated wild geese, one of which would frequently follow him in his morning walks, side by side with Jowler: they seemed to live together on the best terms. Unfortunately the servant neglected one night to confine them, according to custom; the next morning the feathers of the favourite goose were found scattered in a small field adjoining to the grounds. The dog was soon after found concealed in a corner of the wood-yard, and, on his master looking at him, exhibited evident signs of conscious guilt: his master took him to the field, and pointed out to him the feathers: the dog, staring at him, uttered a loud growl, and ran away with all the speed of which he was capable; nor could he bear his master's sight for some days afterwards. At another time, the author had three young sheep, for whom in the day-time the dog seemed to affect the utmost indifference: the servant neglected one evening to take them into their shed, and to confine the dog; and the next morning the sheep were found stretched in the back yard lifeless, and without any other mark of violence than a small wound in the throat, from which the dog had sucked their blood. It is a remarkable circumstance that the Newfoundland dog, when pursuing a flock of sheep, will single out one of them, and if not prevented, which is a matter of considerable difficulty, will never leave off the pursuit until he has mastered his intended victim, always aiming at the throat; and after having sucked the blood, has never been known to touch the carcase."

X. *Spontaneous Ptyalism accompanied by a diminished Secretion of Urine.* By Dr. Prout.

The subject of this singular case was a woman upwards of 60 years of age. The ptyalism was constant and excessive, amounting to several pints in the course of the day, while the urine was reduced to a few ounces. Its source appeared to be the whole of the apparatus destined to secrete saliva. The woman had lost her appetite, and her strength appeared reduced, but in other respects nothing remarkable was observed.

The taste of the fluid was described as *urinous*. It was opalescent, very slightly ropy, and foamed when agitated. Its specific gravity was 1005.5. It restored the blue colour to reddened litmus paper, and faintly reddened turmeric paper. Hence it contained a free alkali, to which was doubtless owing the *urinous* taste above-mentioned.

When exposed to heat, it coagulated. The soluble salts of lead, mercury, and silver, when added to it, caused precipitates. The addition of the mineral acids also caused precipitates. Even dilute acetic acid produced a copious precipitate, but when prussiate of potash was afterwards added, no precipitate took place. Hence the animal matter, though evidently retained in solution by the free alkali present, was not albumen; but appeared to be the peculiar matter secreted by the salivary glands, perhaps a little altered in its nature.

One thousand grs. evaporated to dryness at a temperature between  $212^{\circ}$  and  $300^{\circ}$ , left 8.65 grs. which were found to consist of

Animal matter above-mentioned .....	3.33
Animal matters soluble in alcohol, and similar apparently to what are usually found in the blood .....	1.06
Sulphuric acid. ....	0.90
Muriatic acid. ....	0.75
Phosphoric acid .....	0.06
Alkaline matter, consisting partly of soda and partly of potash .....	2.55
	<hr/> 8.65

With respect to the relative proportions of the potash to the soda, or how they were distributed among the acids, I did not attempt to ascertain. But it is evident that the acids were not sufficient to neutralize the alkalies present, which accounts for its sensible properties, as detailed above. When the animal matters were burned, they left a minute quantity of the earthy phosphates.

The urine of this woman was of an amber colour, slightly opaque. Its specific gravity was 1013.1. It contained crystals of uric acid, and reddened litmus paper more strongly than usual. It contained much less urea than natural, but a large proportion of a brown animal substance, which appeared to render it very-prone to decomposition, especially when exposed to heat.

This case occurred to my friend Dr. Elliotson at St. Thomas's hospital. With the view of increasing the flow of urine, diuretics were given. These produced the desired effect. The urine was rendered more natural and copious; while the salivary discharge was proportionally diminished.

It is not unlikely that cases of this nature have been mistaken for *discharges of urine from the mouth*. In the present case, it appears that the woman herself considered that the fluid came from the stomach, and was of an urinous nature. She had laboured under similar attacks before, but they were less severe, and after some time had ceased spontaneously, and her appetite and strength had returned as usual.



## ARTICLE XII.

*Astronomical, Magnetical, and Meteorological Observations.*

By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*

**Latitude  $51^{\circ} 37' 42''$  North. Longitude West in time  $1^{\circ} 20.7''$ .**

### *Astronomical Observations.*

Oct. 24. Emersion of Jupiter's third	{	9 <sup>h</sup> 19' 39"	Mean Time at Bushey.
satellite .....		9 20 40	Mean Time at Greenwich.

*Magnetical Observations, 1819. — Variation West.*

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
Oct. 1	8h 35'	24° 31'	42''	1h 30'	24° 40'	10''	Owing to the shortness of the days, evening observations discontinued.	
2	8 35	24 30	41	1 35	24 40	06		
3	8 35	24 31	10	1 40	24 41	04		
4	8 35	24 30	24	1 10	24 40	55		
5	8 40	24 31	10	1 20	24 41	01		
6	8 35	24 33	42	1 20	24 42	26		
7	8 40	24 38	25	1 20	24 46	29		
8	8 40	24 36	10	1 10	24 43	44		
9	8 35	24 35	44	1 20	24 42	46		
10	8 35	24 32	50	1 45	24 39	26		
11	8 35	24 35	30	1 35	24 43	25		
12	8 35	24 33	21	1 10	24 41	09		
13	8 35	24 39	44	1 25	24 37	07		
14	8 40	24 33	34	1 15	24 41	22		
15	8 35	24 32	09	1 20	24 39	44		
16	8 35	24 32	12	1 20	24 38	22		
17	8 40	24 48	34	1 15	24 43	01		
18	8 35	24 38	20	1 15	24 43	16		
19	8 35	24 35	40	1 25	24 39	41		
20	8 30	24 36	25	—	—	—		
21	8 35	24 32	47	1 30	24 38	32		
22	8 40	24 31	34	1 20	24 36	34		
23	8 35	24 32	31	1 55	24 38	37		
24	8 35	24 32	12	1 20	24 35	50		
25	8 35	24 32	21	1 25	24 37	05		
26	8 35	24 33	44	1 15	24 38	02		
27	8 35	24 32	26	1 15	24 37	16		
28	8 30	24 29	49	1 55	24 39	52		
29	8 40	24 31	50	1 20	24 40	07		
30	—	—	—	1 20	24 38	27		
31	8 30	24 32	14	1 30	24 38	30		
Mean for Month.	8 34	24 33 27		1 24	24 40 08			

In taking the mean of the morning observations, that on the 17th is not included, being so much in excess, for which there was no apparent cause.



*Meteorological Observations.*

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
Oct.	Morn....	29.365	64°	73°	S by W		Fine	59°
1	Noon....	29.330	69	65	SSW		Cloudy	69
	Even....	—	—	—	—		—	—
	Morn....	29.252	61	87	SSW		Very fine	58½
2	Noon....	29.278	67	63	SW		Very fine	68
	Even....	—	—	—	—		—	—
	Morn....	29.272	59	87	S by E		Cloudy	56
3	Noon....	29.228	63	72	SW by S		Showery	63½
	Even....	—	—	—	—		—	—
	Morn....	29.100	56	88	WSW		Showery	52
4	Noon....	29.100	59	72	NW		Showery	62½
	Even....	—	—	—	—		—	—
	Morn....	29.468	42	73	NNW		Clear	38
5	Noon....	29.545	50	59	NNW		Very fine	50½
	Even....	—	—	—	—		—	—
	Morn....	29.669	45	73	W by S		Cloudy	39½
6	Noon....	29.580	51	69	WSW		Cloudy	55½
	Even....	—	—	—	—		—	—
	Morn....	29.477	55	78	W		Cloudy	45½
7	Noon....	29.450	58	75	W		Cloudy	59½
	Even....	—	—	—	—		—	—
	Morn....	29.580	57	82	W		Cloudy	54½
8	Noon....	29.593	62	75	SW		Showery	63½
	Even....	—	—	—	—		—	—
	Morn....	29.500	56	83	S by E		Rain	56
9	Noon....	29.430	61	67	SSE		Cloudy	62½
	Even....	—	—	—	—		—	—
	Morn....	29.345	59	80	SSE		Fine	55½
10	Noon....	29.360	68	63	S		Very fine	68½
	Even....	—	—	—	—		—	—
	Morn....	29.403	62	73	E		Very fine	58
11	Noon....	29.434	71	59	SE		Very fine	71½
	Even....	—	—	—	—		—	—
	Morn....	29.571	62	71	SE		Clear	58½
12	Noon....	29.570	70	64	Var.		Hazy	70
	Even....	—	—	—	—		—	—
	Morn....	29.533	60	93	SW		Foggy	57½
13	Noon....	29.523	63	79	W		Cloudy	65
	Even....	—	—	—	—		—	—
	Morn....	29.651	53	79	W by N		Cloudy	50
14	Noon....	29.658	58	72	NW		Rain	59
	Even....	—	—	—	—		—	—
	Morn....	29.854	52	81	NNE		Fine	47
15	Noon....	29.861	59	60	NE		Very fine	60
	Even....	—	—	—	—		—	—
	Morn....	29.800	50	79	NW		Showery	46½
16	Noon....	29.781	52	62	NW		Cloudy	52
	Even....	—	—	—	—		—	—
	Morn....	29.685	41	70	NNW		Very fine	37½
17	Noon....	29.700	47	62	N by E		Fine	49
	Even....	—	—	—	—		—	—
	Morn....	29.755	41	73	NNE		Clear	37½
18	Noon....	29.763	51	60	N by E		Cloudy	52½
	Even....	—	—	—	—		—	—

*Meteorological Observations continued.*

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
Oct.	Morn....	29.674	42°	74°	W		Cloudy	38
19	Noon....	29.588	50	65	SW		Fine	52
	Even....	—	—	—	—		—	42
20	Morn....	29.200	52	92	SW by S		Rain	54½
	Noon....	29.119	—	88	SSW		Rain	38
	Even....	—	—	—	—		—	39½
21	Morn....	29.042	39	82	N by W		Showery	26
	Noon....	29.062	36	80	NW by W		Snow	41½
	Even....	—	—	—	—		—	34½
22	Morn....	29.962	31	88	W by N		Snow	48½
	Noon....	29.950	40	69	WNW		Cloudy	34½
	Even....	—	—	—	—		—	44½
23	Morn....	28.893	37	78	W		Very fine	30½
	Noon....	28.852	47	68	WNW		Cloudy	42½
	Even....	—	—	—	—		—	34
24	Morn....	28.869	38	80	NW		Very fine	45
	Noon....	28.895	44	75	NNW		Cloudy	80
	Even....	—	—	—	—		—	42½
25	Morn....	28.954	32	75	NW		Cloudy	32
	Noon....	28.992	39	69	WNW		Cloudy	45
	Even....	—	—	—	—		—	30½
26	Morn....	29.120	39	82	NNW		Showery	36½
	Noon....	29.180	45	65	NW by N		Very fine	43½
	Even....	—	—	—	—		—	41
27	Morn....	29.353	33	73	N		Very fine	36½
	Noon....	29.330	41	70	NE by N		Showery	45½
	Even....	—	—	—	—		—	45
28	Morn....	29.328	35	77	NE by N		Cloudy	30½
	Noon....	29.317	45	65	NE by N		Very fine	41
	Even....	—	—	—	—		—	36½
29	Morn....	29.083	33	83	NE		Foggy	43½
	Noon....	29.030	39	81	NE		Showery	39½
	Even....	—	—	—	—		—	47
30	Morn....	29.079	—	91	NE		Rain	
	Noon....	29.100	41	93	NE		Rain	
	Even....	—	—	—	—		—	
31	Morn....	29.352	45	89	ENE		Rain	
	Noon....	29.370	46	87	ENE		Rain	
	Even....	—	—	—	—		—	

Rain, by the pluviometer, between noon the 1st of October and noon the 1st of November, 1.61 inch. Evaporation, during the same period, 2.28 inches.

On Sunday the 17th, at nine in the evening, the northern lights were uncommonly vivid, and it was on the morning of that day the magnetical variation was so great. During the night of the 21st, a heavy fall of snow took place, accompanied by a violent wind; the leaves being on the trees caused such an accumulation of weight, that for many miles round serious damage has been sustained.

## ARTICLE XIII.

## METEOROLOGICAL TABLE.

1819.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
		Max.	Min.	Max.	Min.			
10th Mo.								
Oct. 1	S	29.81	29.76	75	57	—	—	68
2	S W	29.80	29.77	71	57	—	—	81
3	S	29.80	29.63	69	52	—	—	75
4	S W	29.98	29.63	66	36	—	10	74
5	N W	30.12	29.98	51	39	—	—	68
6	W	30.17	30.01	56	49	57	—	67
7	W	30.09	30.00	61	55	—	—	79
8	W	30.09	30.03	67	57	—	—	80
9	S W	30.03	29.87	67	55	—	—	76
10	S E	29.88	29.87	77	54	—	—	73
11	E	30.04	29.88	77	47	40	—	75
12	S W	30.04	30.02	77	57	—	—	79
13	S E	30.15	30.02	68	46	—	—	84
14	N W	30.33	30.15	63	44	—	—	78
15	N E	30.33	30.30	62	39	—	—	78
16	N W	30.30	30.22	56	37	34	—	79
17	N	30.28	30.22	49	34	—	—	69
18	N	30.28	30.23	56	26	—	—	72
19	W	30.23	29.76	56	34	—	—	78
20	S W	29.76	29.61	58	40	—	35	83
21	N W	29.61	29.42	42	30	—	70	74
22	N W	29.46	29.41	43	32	—	—	81
23	W	29.41	29.34	51	30	38	02	80
24	N N W	29.43	29.35	48	34	—	—	69
25	N W	29.69	29.43	57	27	—	—	70
26	N W	29.88	29.69	47	22	—	—	85
27	N W	29.87	29.84	45	26	—	02	77
28	N	29.84	29.68	50	25	—	—	68
29	N E	29.68	29.65	43	35	—	26	78
30	N E	29.88	29.65	48	42	—	56	80
31	N E	29.90	29.88	48	45	25	08	98
		30.33	29.34	77	22	1.94	2.09	67—98

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Tenth Month.*—1. *Cirrus, Cirrocumulus.* 2. Cloudy. 3, 4. Cloudy: windy. 5. An immense collection of swallows in the evening. 6—8. Overcast. 9—11. Fine, with the lighter modifications. 12. Foggy morning. The aurora borealis is said to have appeared at night. 16. This day the *Cirrus* cloud prevailed in an unusual manner, increasing in density until the oblique depending tufts showed dark against a completely overcast sky above: the direction of these tufts was towards the west. 17. Fine: aurora borealis in the evening. (This was observed at Tottenham by my friend William Phillips.) 18. The first hoar-frost: the tender garden plants, as kidney beans, &c. killed. 19. Overcast. 20. Drizzling, p.m. 21. After rain and a little sleet, it began to snow about noon, falling in very large flakes, thick and rapidly for an hour, and covering the ground: some rain followed. In the evening the wind rose, and it blew hard in the night from about NNW. At midnight came on a second heavy fall of snow, which continued till six, a.m. the 22d; and though at first much of it melted, it lay in the morning full three inches deep. This day, of course, presented the appearance of mid-winter, with the single exception of the foliage still remaining on the trees, which, mingled with an enormous burden of snow, presented a very singular and grotesque appearance. The somewhat moist state of the snow (as happens also when it falls late in the spring) caused it to be very adhesive, and the frost cementing the masses, the very walls and fences remained thickly coated for some hours; while that on the trunks of trees indicated with precision the quarter from whence it came. Much damage was done, by the breaking down of large limbs from fruit and forest trees in all directions. 23. Snow remained in places the whole of the day; notwithstanding which I observed swallows about at Stamford Hill. 24. A very white frost this morning: temp. 31° at eight, a.m. at Tottenham: day windy, bleak, cloudy. 27, 28. Hoar frost. 30, 31. Rainy.

## RESULTS.

Winds: N, 3; NE, 4; E, 1; SE, 2; S, 2; SW, 5; W, 5; NW, 9.

## Barometer: Mean height

For the month. ....	29.862 inches.
For the lunar period, ending the 10th. ....	30.004
For 13 days, ending the 2d (moon south) ....	29.933
For 14 days, ending the 16th (moon north) ....	30.003
For 13 days, ending the 29th (moon south).....	29.732

## Thermometer: Mean height

For the month.....	49.475°
For the lunar period (as above) .....	59
For 31 days, the sun in Libra.....	54.34

Hygrometer: Mean for the month . . . . . 76

Evaporation. .... 1.94 inches.

Rain..... 2.09

By a second gauge..... 2.18

Stratford, Eleventh Month, 20, 1819.

L. HOWARD.

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